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THE

EDINBURGH

PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF

THE PROGRESS OF DISCOVERY IN NATURAL PHILOSOPHY, CHEMISTRY, NATURAL HISTORY, PRACTICAL MECHANICS, GEOGRAPHY, NAVIGATION, STATISTICS, AND THE FINE AND USEFUL ARTS,

FROM

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THE
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SIR,

IN compliance with your request, I have analysed the Apophyllite of Faro, whose action upon polarised light presents such remarkable peculiarities, that you have considered it as a new mineral species, and given it the name of *Tesselite*.

The investigations in which you are occupied, respecting the action of crystallised substances upon light, are of such high importance, that I feel it my duty to do every thing in my power to furnish you with the analytical data which you require; and though the result which I have obtained in the analysis of the *Tesselite*, does not appear to confirm your conjecture respecting the difference between its composition and that of the other apophyllites *, I shall communicate to you the experiments which I have made upon this mineral.

The specimens of Tesselite with which Sir George Mackenzie had the goodness to supply me for analysis, though very large as mineralogical specimens, yet yielded only a small quantity of the pure crystals of the Tesselite, of which I could not collect more than a gramme, or $18\frac{2}{3}$ grains.

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* See the following Article.—ED.

I have analysed it in the same manner as the apophyllites of Uto and of Fassa, the analyses of which are published in the *Afhandlingar i Fysik*, vol. vi. p. 181. and it has given me the same result as the apophyllite of Uto even in the thousandth parts. But in examining on the present occasion the Apophyllites with the most scrupulous attention, I have discovered a principle which had hitherto escaped notice, viz. *Fluoric Acid*, which can be easily detected by the blowpipe.

I have tried different methods of determining the quantity of fluoric acid in this mineral, and I believe I have at last succeeded. The Apophyllites lose on a red heat from 16 to 16.2 hundredths of their weight; but when exposed to a strong incandescence, their loss amounts to 16.7. At a still stronger heat, where the mineral is reduced to a spongy glass, its weight is not diminished. I have repeated the analysis of the Apophyllite of Uto in the following manner. The mineral, when coarsely pulverised, was decomposed by cold muriatic acid, diluted with water. It was soon converted into a gelatinous mass, by which all the fluid was absorbed. Having washed it with water, and filtered it, 51.18 hundredths of the weight of the apophyllite remained on the filter, and was pure Silex.

The liquid being super-saturated with caustic ammonia, gave an abundant precipitate, which, when collected on the filter, washed, and dried, weighed 4.82 hundredths, and the nature of which remained to be determined.

The rest of the liquid gave, by means of oxalate of ammonia, 21.71 of Lime, and, after the evaporation of the liquid, and the exposure of the saline matter to the fire, 8.32 parts of Muriate of Potash, equivalent to 5.27 of pure Potash.

In order to examine better the substance precipitated by the caustic ammonia, I prepared a greater quantity of it. When put in contact with sulphuric acid, it gave a brisk effervescence. The gas discharged smelt of fluoric acid, but acted upon glass only feebly. When 100 parts of this mass was dried in the fire, it was deco ans of concentrated sulphuric acid, and the gas ir lixivium of carbonate of soda.

There it deposited silex abundantly: the neck of the retort was not corroded, and it shewed only slight traces of silex deposited by the gas. The alkaline liquid, when evaporated to dryness, and redissolved in water, left 19 parts of undissolved Silex. The alkaline solution, when supersaturated with muriatic acid, in a platinum dish, and heated slightly for some hours, to drive off the carbonic acid, was afterwards neutralised by the ammonia, of which a small excess was added, but no precipitate was formed. By adding muriate of lime, Fluate of Lime was formed, which, when washed and dried, weighed 65.67 = 18.26 parts of Fluoric Acid. The residue in the retort being exposed to the fire, to deprive it of its excess of sulphuric acid, left 150 parts of Gypsum, equivalent to 62.25 parts of pure Lime. The precipitate thus analysed, was therefore composed of

Lime, - - -	62.25	or	63.75
Silex, - - -	19.00		17.80
Fluoric Acid, - -	18.26		18.45
	99.51		100 0

as calculated from the formula $CS^2 + 3CFl$.

The 4.82 parts precipitated from the solution of apophyllite by the caustic ammonia, were therefore a true Fluo-silicate of Lime, and contained 3 parts of Lime, 0.95 of Silex, and 0.87 of Fluoric Acid.

In order that nothing might be neglected which could contribute to a perfect knowledge of the composition of *Tesselite*, I repeated the preceding analyses upon 312 millegrammes which still remained of the mineral. I shall here compare the results of the analyses of the *Apophyllite* and of the *Tesselite* with one another, and with the result obtained by the old analytical method; that is to say, where the fluoric acid was partly disengaged in evaporating the solution to dryness, and partly combined with the lime.

	Tesselite.	Apophyllite of Uto.
Silex, - - -	51.76	51.18
Fluo-silicate of Lime,	3.53	4.82
Lime, - - -	22.73	21.71
Potash, - - -	5.31	5.27
Water, - - -	16.20	16.20
	99.53	99.18

By reducing the Fluo-silicate of Lime into its elements, we obtain

	Tesselite.	Apophyllite of Uto.	Old Method.
Silex, - -	52.38	52.13	52.90
Lime, - -	24.98	24.71	25.21
Potash, - -	5.27	5.27	5.27
Fluoric Acid, -	0.64	0.82	...
Water, - -	16.20	16.20	16.20
	99.47	99.13	99.38

It appears from the experiments of MM. Bonsdorff and Rose, that the Fluoric Acid is often found in small quantities in minerals, without having any effect in altering their external characters; but we are ignorant of the kind of combination which it forms in these minerals. I have made some attempts to discover it, which I shall communicate to you at another opportunity; and I believe that I have found that the fluoric acid combines with the silicates of certain bases, such as potash, lime, barytes, without displacing the silex, provided that the acid does not exceed what is necessary to neutralise the base, for an excess would give rise to the fluo-silicic gas. It follows, therefore, that a small quantity of fluoric acid may be absorbed by a silicate of potash or lime, without having its constitution sensibly altered. The fluo-silicate found in the apophyllites by analysis, does not exist as such in the mineral. It is a combination with an excess of base, formed by the analytical operations. Besides, I will not decide whether the fluoric acid is combined with the silicate of lime, or with that of the potash, though the first appears to be the most probable, considering the excess of silica combined with the potash, the formula which expresses the composition of apophyllite being $KS^6 + 8CS^3 + 16Aq$.

It appears to me, that mineralogical characters drawn from optical phenomena presented by transparent crystallised minerals, may be derived from several different sources. 1. From the nature of their elements. 2. From the number of atoms of each element, on which depends the crystalline system to which the mineral belongs. 3. From accidental mixtures, often inconsiderable, which alter, in an essential manner, the transparency, the form, [illegible] crystallisation. These are what are called in artifici [illegible] rities. To these we may add another, viz. The variable transparency of different parts of the same crystal. You have no doubt observed, that several salts, among others the Sulphate of Potash and the Nitrate of Lead, yield crystals sometimes transparent and sometimes opaque, and of a milky whiteness, without this difference being accompanied with any alterations in the proportions of their elements. Among the crystals of the Faro apophyllite, there are some which exhibit milky-white parts, arranged in a regular manner, often in the form of a diagonal cross, and easily discernible to the naked eye. Differences of this kind, however great be the influence which they exercise upon light, can never constitute differences of species in mineralogy,—differences which can only be founded on a real diversity of composition. To distinguish among the optical phenomena produced by accidental circumstances, and those which are derived from a difference in the elements, or in the number of their atoms, is to carry to its *maximum* the employment of optical phenomena as distinctive characters in mineralogy.

Presuming that, in the prosecution of your optical researches, you are desirous to know the composition of the other minerals which you have had the goodness to send me, I have analysed them, and shall now communicate to you the results.

I began with the examination of the substances which accompany the Faroe apophyllite. They are inclosed in cavities more or less great in the lava which is their gangue. The apophyllite is found only in the larger cavities: it and the stilbite appear on the anterior surface of the cavities, and have therefore been last crystallised. Below and round these two, the interior is covered with a mamillated mineral, which, in its interior, exhibits a radiated and concentric crystallisation. Its colour borders a little upon yellow. Below this last, and in immediate contact with the lava, is a whiter substance, without any determinate marks of crystallisation, though its fracture is grained, and exhibits irregular facets. This mineral has crystallised first. It also fills all the little cavities; but in the centre of larger cavities, we find the radiated mineral, which, in the largest cavities, covers entirely the grained mineral, by a stratum considerably thicker than that of the latter. These two minerals have a great relation with the Mesolite; and in order to distinguish them, I shall call the upper stratum Mesole, and the lower grained stratum Mesoline.

These substances were analysed in the following manner. The mineral, in as great a state of purity as possible, was reduced into powder, and afterwards divided into two parts, one of which was exposed to a fire, at first moderate, and afterwards incandescent. It was then observed if the loss arose from the second ignition, and by this means it is easily determined whether or not a mineral contains a volatile acid; for the water disengages itself from it at a temperature which is not yet red, provided it has been continued for a sufficient length of time, and the acids are not volatilised but with a white heat.

The other part of the powder was decomposed by concentrated muriatic acid, and the gelatinous mass was dried with a gentle heat, in order to render the silex insoluble. It was afterwards moistened anew with concentrated muriatic acid, and, after two hours, it was washed with water. The silex remained on the filter, and the clear liquid was precipitated by caustic ammonia, added in small excess in a glass, to which the carbonic acid of the atmosphere could have no access. It was then decanted, and the alumina washed on the filter. This earth was then brought to a red heat, weighed, and afterwards dissolved in concentrated muriatic or sulphuric acid. It commonly left a little silex undissolved, which was deducted from the weight of the alumine. Caustic potash was afterwards added to the solution, till it re-dissolved the alumine; and if on this occasion there remained an insoluble residue, its weight was also deducted from that of the alumine precipitated, the weight of which is thus obtained with more exactness than if we sought to determine it after having dissolved the alumine in the potash. The lime was precipitated by means of the oxalate of ammonia, the remaining liquid evaporated to dryness, and the saline mass heated, so as to volatilise the sal ammoniac, and give the alkali in the form of a muriate. The oxalate of lime was heated to redness, and the small quantity of carbonic acid which it could thus lose, was restored to it by some drops of carbonate of ammonia. It was then well dried, and weighed in the form of carbonate of lime.

1. *Mesole*, or the radiated mineral. The mesole lost in a red heat from 12.7 hundredths of its weight, and at a white heat 13.3. The increase is owing to a small quantity of carbonic acid, of which it contains variable proportions. In a fine powder it gives at first a slight effervescence in muriatic acid, but in small grains it is not perceptible. I have found its composition to be,

Silex,	42.60	Contains of Oxygen,	21.40	No. of Equivalents,	15
Alumine,	28.00		13.07		9
Lime,	11.43		3.19		2
Soda,	5.63		1.44		1
Water,	12.70		11.29		8
	100.36				

In examining this result, it appears, that the lime exceeds a little that which it ought to be, in order to contain two times the oxygen of the soda, which arises from the presence of a small quantity of carbonate of lime. Neglecting this last, which is only an accidental mixture, we find for the composition of mesole the following formula, $NS^2 + 2CS^2 + 9AS + 8Aq$. The mesole differs from the mesolite in this, that in the latter the soda and lime, as we shall presently see, are combined with three equivalents of silex, $NS^3 + 2CS^3$. This difference induced me to repeat the analysis, with the mesole taken from another specimen, and I found it confirmed. The nature of the alkali was examined in the following manner. The weights of the muriate being determined, I dissolved them in a very little water, and I mixed with it a solution of the double muriate of soda and platinum, and when it did not form a precipitate, I dried the mixture. I then dissolved the dry mass in alcohol, which leaves undissolved the muriate of potash and platinum, if the mineral contains any. If on this occasion we make use of the muriate of platinum, which is added in excess, the alcohol carries away a little of the salt, from the base of potash rendered soluble by the excess of the muriate of platinum. The mesole has given me only insignificant traces of the double muriate of potash and platinum.

2. *Mesoline*, or the granular mineral. It is very difficult to obtain fragments of this mineral entirely pure, as the mesole adheres to it strongly on one side, and the lava on the other.

In order to avoid the first, I selected the mesoline, which occurs in the smallest cavities in the form of the head of a pin, but whose exterior surface still contains lava. This mineral loses on the fire 18.19 hundredths of its weight of water. Its composition gives

Silex	47.50	Contains of Oxygen,	23.88	No. of Equivalents,	18
Alumine,	21.40		10.00		9
Lime,	7.90		2.20		2
Soda,	4.80		1.23		1
Water,	16.19		16.16		14
	97.79				

This result contains an excess of silex and of soda. The first is derived from an accidental mixture of lava, which makes the silex obtained by the analysis greyish. The last is only apparent because the soda contains potash, of which I have not thought it necessary to determine the precise quantity, since it is too small to alter the calculated result. In neglecting it, the composition of this mineral may be represented by $NS^3 + 2CS^3 + 9AS^3 + 14Aq$.

3. *Mesolite* or Needlestone from Faroe. This mineral loses in the fire 12.3, and its analysis is as follows:

Silex,	46.80	Contains of Oxygen,	23.54	No. of Equivalents,	18
Alumine,	26.50		12.37		9
Lime,	9.87		2.76		2
Soda,	5.40		1.38		1
Water,	12.30		10.93		8
	100.87				

This result is precisely the same which MM. Gehlen and Fuchs obtained from their analysis, (see *Ann. of Phil.* vol. xvi. p. 406.); and the formula $NS^3 + 2CS^3 + 9AS^3 + 8Aq$, does not differ from the mesoline, but in containing 6 proportions less of water.

4. *Mesotype from Kilpatrick* *.—This is probably the same substance which that learned mineralogist Mr Brooke first distinguished from the mesotype of Auvergne, by giving it the name of Thomsonite, and which Dr Thomson has analysed with his usual accuracy. The Thomsonite loses in the fire 13.1 of its weight, and c [illegible]

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* Dr Brewster wa [illegible] cimens of this mineral which he sent to M. Berzelius, to the l [illegible] on of Glasgow.

Silex,	38.30	Contains of Oxygen,	19.26	No. of Equivalents,	16
Alumine,	30.20		14.10		12
Lime,	13.54		3.76		3
Soda,	4.53		1.16		1
Magnesia,	0.40				
Water,	13.10		11.55		10
	100.07				

This mineral contains only traces almost imperceptible of potash. The alumine which I obtained from it, when precipitated by ammonia, presented a phenomenon which I had not observed before, and with which I was much struck. After having weighed it, I introduced it into a phial where there were some drops of moisture. The phial became warm. I then heated the phial, to volatilise the moisture, and when the alumine was entirely cooled, I allowed some drops of water to fall upon it, when it again became warm. I supposed that this effect was produced by a little lime in the alumine, but I could not find any foreign substance, excepting a very small quantity of magnesia. I next prepared a mixture of alumine and magnesia, nearly in the same proportion, and I found that when these two earths are precipitated together, they acquire the property of heating one another, when, after drying them in the fire, they are moistened with water,—a property which neither of them possesses in an insulated state. When we dissolve the alumine which contains the magnesia, it forms at last a white powder, which appears insoluble in acids, and which appears to be an aluminate of magnesia *(Spinelle)*. By boiling the solution for some time, it is at last completely dissolved.

The formula which expresses the composition of Thomsonite is $NS + 3CS + 12AS + 10Aq$. As there are *Paranthines* which contain soda, we may say that Thomsonite is a *Paranthine* with water of crystallisation. We are, however, still ignorant of the ratio of the lime to the soda in the paranthines which contain the latter.

5. The mineral sent by Mr Allan under the name of *Chabasie*, but without a locality, has been analysed by my friend Mr Arfwedson, who found it to consist of

Silex, - -	49.07	Contains of Oxygen,	24.68	No. of Equivalents,	8
Alumine,	18.90		8.83		3
Soda, containing a little Potash,	12.19		3.11		1
Water,	19.73		17.55		6

The presence of potash in the soda, makes it appear that there is a small excess of this last. The formula, which expresses the composition of this mineral is then $\left.\begin{matrix} NS^2 \\ KS^2 \end{matrix}\right\} + 3\ AS^2 + 6\ Aq.$, the first term of which will require some explanation. When it appears that a small quantity of one base replaces a portion of another that is wanting, without its appearing that this replacement is made according to chemical proportions, we consider it as purely mechanical, and arising from the isomorphous nature of substances crystallised together. In this case we make a single term of the substances which are supposed to replace one another mutually, by setting the one above the other, the sum of the oxygen in the two bases being thus taken as the oxygen of the single one. This stone is composed of fixed elements, precisely like Analcime, from which it differs only by a triple proportion of water, and in which M. Arfwedson has also found potash mixed with soda. As a point of comparison, M. Arfwedson has analysed also the primitive chabasie of Fassa, which was given me for analysis by M. Haüy. It is composed of

Silex, -	48.38	Which contains of Oxygen,	24.37	No. of Equivalents,	8
Alumine,	19.28		9.00		3
Lime, -	8.70		2.44		1
Potash, -	2.50		0.43		
Water and Loss,	21.14		18.81		6
	100.00				

The formula which results from this analysis is $\left.\begin{matrix} CS^2 \\ KS^2 \end{matrix}\right\} + 3\,AS^2 + 6\ Aq.$ You see, then, that the kind of combination is the same among these minerals, but that the one contains soda, whereas the other contains lime, a difference analogous to that between Scolezite and Mesotype.

I shall add here the results of an analysis, which I made some years ago, o found in a primitive mountain at Gustafsberg. It

Silex,	50.65	Which contains of Oxygen,	25.44	No. of Equivalents,	9
Alumine,	17.90		8.36		3
Lime,	9.73		2.73		1
Potash,	1.70		0.29		
Water,	19.90		17.69		6

The result of this analysis differs from that of M. Arfwedson, by an additional equivalent of silex; but the mineral was formed upon a crust of quartz, which often penetrates the crystals, and I am persuaded that this circumstance is the cause of the excess of silex.

The *Red Stilbite* sent also by Mr Allan, is the Stilbite Anamorphique of Haüy [*]. M. Walmstedt, Professor of Chemistry at Upsal, has proved, that the Stilbite Anamorphique and Octodecimale, have a composition differing essentially from that of the ordinary Stilbite [†]. The following is a comparison of the two analyses,

	Common Stilbite by Hisinger.	Stilbite Anamorphique by Walmstedt.
Silex, - -	58.00	59.90
Alumine, - -	16.10	16.87
Lime, - -	9.20	7.19
Water, - -	16.40	13.43

The formula of the first is $CS^3 + 3\,AS^3 + 6\,Aq.$, and that of the last $CS^3 + 4\,AS^3 + 6\,Aq.$ The different quantities of water which these minerals contain, afford an easy method of distinguishing them, since the one loses in the fire 3 hundreths of its weight more than the other.

From these analyses, you will observe that the zeolites deserve to be examined by chemists with much more attention than they have hitherto received. I am persuaded that they contain a great number of different combinations, which are at present confounded.

I am, Sir, yours, &c.

STOCKHOLM,
9th April 1822.

BERZELIUS.

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[*] The *Heulandite* of Mr Brooke. See this *Journal*, Vol. VI. p. 112.—ED.

[†] Mr Brooke has already, from crystallographic characters, separated the ordinary Stilbite from the two varieties above mentioned, and it is satisfactory to find the conclusions of this able mineralogist so strikingly confirmed by chemical analysis. See this *Journal*, Vol. VI. p. 112.—ED.

ART. II.—*Observations on the Relation between the Optical Structure and the Chemical Composition of the Apophyllite and other Minerals of the Zeolite Family, in reference to the preceding Analyses of M. Berzelius.* By DAVID BREWSTER, LL. D. F. R. S. L., & Sec. R. S. Edin.

THE analyses described in the preceding paper, and obtained by the first analytical chemist of the present age, possess a high degree of interest, when considered merely in reference to mineralogy and analytical chemistry. The relation, however, which they bear to the optical or polarising structure of mineral bodies, is perhaps still more important, as they become established data, to which we may confidently appeal in establishing the universal infallibility of optical analysis, as affording essential and distinguishing characters of mineral bodies.

In the preceding letter, M. Berzelius has proved, beyond a doubt, that in the *present condition of chemical analysis*, the *Tesselite* possesses the same ingredients as the *Apophyllite* of Uto; and he is of opinion, that, as it is *chemically* the same substance, it ought not to be regarded as a separate mineral species, and distinguished by a separate name.

In the present case, therefore, the results of *Chemical analysis* are apparently set in opposition to those of *Optical analysis*, and it is incumbent upon those who confide in the latter to give some plausible explanation of this apparent discordance.

There are at present only THREE *scientific methods* by which new mineral species can be discovered,

1. The *Method of Chemical Analysis*, by which we discover the different elements of which minerals are composed, and the different proportions in which these elements are combined.

2. The *Method of Crystallographic Analysis*, by which we ascertain the mechanical structure of crystals by cleavage, and thus obtain for each mineral species a primitive form, to which all its secondary forms may be referred.

3. The *Method of Optical Analysis* *, which enables us to determine the prin [illegible] orm of minerals from the number of

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* In M. Berzelius's [illegible] le *Mineralogie*, published in Paris in 1819, p. 307., he has s[illegible] mistake, that the experiments on the

their axes of double refraction; and to distinguish mineral species by differences in the position of the axes of double refraction;—in the nature of these axes, whether *positive* or *negative*;—in the absolute intensity of their action upon light, when the crystals have one or two axes;—in the dispersive power of the axes;—in the relative intensity of the axes, when they have two;—or in the imperfect equilibrium of the axes when they have three;—and by detecting in polarised light compound and remarkable structures, which neither the crystallographer nor the chemist have the means of discovering.

Of these *three methods of analysis*, we conceive the *crystallographic method* to be the *least perfect*; the *chemical method* next to it; and the *optical method* decidedly superior to both.

It appears from the beautiful experiments of M. Mitscherlich, that there are certain bases which he calls *isomorphous*, such as the neutral phosphates and arseniates, which are substituted, as it were, in place of each other in mineral bodies, while the external form of the mineral remains the same. If this result is rigorously true, the crystallographic method must be considered as incapable of affording essential characters of all such mineral

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relation between the optical structure *and Chemical Composition of* different kinds of mica were begun by M. Biot. M. Berzelius has, no doubt, been misled by the circumstance of M. Biot's paper appearing in the Memoirs of the Institute for 1816, although it was neither written till 1818, nor read till the 22d June 1818, and of its not containing the slightest allusion to my previous labours. This singular anachronism in the Transactions of a public body, has a tendency to do much mischief, as no person is likely to suspect that Memoirs for 1816 will contain papers written in 1818. In the Transactions of the Royal Society of London, the effect is quite the reverse, as the volumes never can contain papers of a subsequent date. In the volume for 1818, for example, the paper of mine, which stands in contrast with that of M. Biot bears the date of June 1. 1817, and was in Sir Joseph Banks's possession in December 1817. The experiments themselves were signed by Sir George Mackenzie on the 24th November 1816;—the 2d December 1816, and the 6th January 1817. The general results of these experiments were also communicated to M. Biot personally, when he was in Scotland, in the summer of 1817. If M. Berzelius will therefore have the goodness to compare my papers in the *Phil. Trans.* 1818, p. 223, 224, 225, 231, and the remarks in this *Journal*, Vol. V., p. 1., with M. Biot's *Traité de Physique*, vol. iv. p. 553, 554., published in 1816, and his Memoir on Mica, published in 1818, he will form a correct opinion on the point under consideration. In an early number of this work, we shall have an opportunity of rectifying these and other errors relative to this branch of optics, by the publication of various documents.

bodies, and must, on this account alone, be held to be inferior to the other two.

The imperfections of *chemical analysis* are so well known, and so candidly admitted by the most distinguished chemists, that it would be an unprofitable task to enumerate the discordant results obtained even by masters of the analytic art. It may be sufficient to state, that the chemist examines a mineral in its ruins, and after its parts have been separated by the disintegrating power either of heat or acids. He may determine the quantity, but he is frequently ignorant of the nature of those volatile parts which escape during the destroying process. The fluoric acid, for example, in certain micas eluded the penetration of Vauquelin, and the same gas was not discovered by Berzelius in his first analyses of apophyllite.

With regard to the method of *optical analysis*, the case is quite different. We examine the mineral in its perfect and undisturbed condition. We determine its various properties, as modified by the elements of which it is composed, by the proportions in which they combine, and by the mechanical or crystallographic structure into which they are arranged. We therefore determine properties, and measure actions, and observe structures, which vary with the elementary parts of the mineral, as well as with their mode of combination; and though we do not know either the names or proportion of the elements whose agency is under our observation, yet we are in reality observing the direct effects of chemical composition and crystallographic structure. In proof of these positions, I might refer in general to the universality of the law which I have established between the primitive forms of crystals and the number of their axes of double refraction, and to the various new mineral species which have been detected by optical analysis alone; but as the papers in which these results are contained, are printed in various works, I shall direct the attention of the reader to the influence of optical analysis in determining the mineral species of the Zeolites, one of the most perplexing families of mineral substances, and upon which the chemist and the crystallographer have exhausted al [illegible] sources of their respective arts.

When Haüy [illegible] *Traité de Mineralogie* in 1804, the two species of [illegible] ESOTYPE embraced the following substances, which have been since found to be distinct minerals,

Stilbite.
Heulandite.
Brewsterite.
Stilbite from Aachen.
Auvergne Mesotype.
Scolezite or Iceland Mesotype.
Thomsonite, or Kilpatrick Mesotype.
Mesolite or Nadelstein from Faroe.
Greenland Mesotype.
Natrolite.
Apophyllite Surcomposé.
Tesselite.

Four of these crystals, viz. the *Stilbite from Aachen*, the *Greenland Mesotype*, the *Tesselite*, and the *Nadelstein from Faroe*, I found to be new minerals, from optical analysis alone; and by the same method I have ascertained that all the other minerals above enumerated, are distinct mineral species. Hence we may conclude, that in every case where the chemical and crystallographic methods have detected new mineral species, the method of optical analysis never fails to give the same result, and to trace in the variations of optical characters every change of form or of composition.

But, while this method must be regarded as superior to all other methods of discriminating minerals, *as long as no instance of failure can be pointed out;* it possesses at the same time an advantage to which neither chemical analysis nor crystallographic dissection can aspire. It enables us to detect and explore those compound forms of dead matter which almost rival the organisations of animal life, and to study those Mosaics of the natural world which have entirely escaped the observation of mineralogists. I do not allude to those hemitrope crystallizations which so frequently occur, but to compound structures which exist within regular geometrical forms,—which are characteristic of the mineral in which they occur,—and which are essentially necessary to its existence. The most remarkable of these structures are the tesselated combinations of *Apophyllite*, the aggregation of prisms which compose a perfect dodecahedral crystal of *Sulphate of Potash*, the composite forms of *Amethyst*, *Chabasite* and *Aplome*, and the still more remarkable assemblage of solids which constitute the *Analcime*.

It may be objected, however, to the optical method of analysis, and with some semblance of justice, that its powers are limited to transparent or translucent minerals. It is no doubt true, that in certain substances, of almost absolute opacity, we cannot discover their axes of double refraction, or examine their action upon transmitted light; but there are very few minerals of this description, from which I have not been able to obtain an optical character, and I have no doubt, that when we are better acquainted with the superficial action of bodies upon light, all these will be embraced by this general method. But while the optical method is limited to this extent, the chemical and the crystallographic methods have also their limitations. In very scarce minerals, which exist only in small specimens, such as the Stilbite from Aachen, the chemist and the crystallographer are often unable to find a sufficient portion for their analyses, while the optical observer can work with the smallest fragments, and still preserve them for the cabinet of the mineralogist. In granular minerals, and those which are disseminated through rocks, the chemist is equally baffled, while the resources of the optical method remain unexhausted.

From the similarity in the composition of the Faroe and Uto Apophyllite, M. Berzelius has concluded, that the first of these substances was not entitled to a separate name. If this distinguished chemist meant to say, that in a *chemical system of mineralogy*, the two substances should be arranged as one, we entirely agree with him; but if he intended to state, that, in a *general system of mineralogy*, the Tesselite should not appear as a separate mineral, and under a separate name, we must object to the adoption of such an opinion. If the chemist exercises the right of distinguishing, in his nomenclature, minerals externally the same, but differing by only a few *per cents.* of some solid or gaseous ingredient, it would be a strange abridgment of the privileges of the optical observer, if he were not permitted to designate by a new name a body which possesses one of the most remarkable structures that has ever come under the notice of the mineralogist. The leading object of mineralogy is to explain the physical properties of mineral bodies; and their artificial classification is to be considered in no other light than as a means of attaining this object. But how can the mineralogist either prosecute his inquirie [illegible] his science, if he does not attach different names t[illegible] rent properties. Were he to follow the chemical method alone, he would confound *Quartz* with *Amethyst,* and extinguish the brilliant reflexions of the diamond under the name of *Mineral Carbon.*

When the chemist determines that two bodies have the same ingredients, he is ignorant of the manner in which these ingredients are combined. If the analysis of a quadruped and a bird indicated the same elements, he could not discover that these elements formed hair and horn in the one, while in the other they displayed themselves in the finest plumage. The chemist, indeed, must fail in detecting structures, because he commences his process by destroying them.

In these observations, we suppose the art of the chemist to be perfect, and that the chemical composition of the two substances which he compares is determined beyond a doubt. But as this is far from being the case, we are disposed to think that when either the crystallographic or the optical methods point out new properties in minerals which the chemist has confounded, he ought to consider these properties as affording at least strong presumptions that his art may be in fault. The history of Arragonite furnishes us with an excellent illustration of this remark; and it is a fact not less important, that the late Reverend Mr Gregor discovered in the Brazilian Topaz * both Potash and Soda, although both these ingredients eluded the sagacity of Klaproth and Berzelius.

M. Berzelius has remarked, that while the optical characters of minerals may be derived from the nature of their element, and the number of atoms of each element, they may arise also from casual mixtures and impurities, and that to employ optical phenomena, produced by such accidental circumstances, is to carry the use of this method to its *maximum.* In this opinion we heartily concur; but we are not aware that any such optical phenomena have ever been proposed, even as subsidiary characters; and we should think little of any system that founded its decisions on the variations of colours or of transparency, or in numerous other accidents of light which are familiar to mineralogists. The system of optical analysis disclaims all such trivial distinctions, and founds its results upon characters as essentially necessary to the existence of the mineral as the most prominent of its chemical elements.

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* The optical properties of the Brazilian Topaz differ essentially from those of Scotland, Saxony, and New Holland. See Dr Paris's Memoir of the Reverend W. Gregor, London 1818, p. 29. Note, and p. 31., for an account of his Analysis of the Brazilian Topaz, and a paper on the Topaz, which will appear in the 3d No. of the *Transactions of the Cambridge Philosophical Society.*

There are some circumstances in the comparison of the Faroe and the Uto Apophyllites, which require to be carefully kept in view, before we pronounce the Tesselated Apophyllite to be chemically the same with that which is not tesselated. I have reason to believe, that the Tesselite exists among the Uto apophyllite, and, therefore, it is not improbable that M. Berzelius may have analysed the Tesselite of Uto. The two kinds of Apophyllite which are set in peculiar contrast with one another by the optical method, are the *Apophyllite surcomposée* from Fassa, and the *Tesselite* of Faroe; and therefore, the analysis of these two crystals, when the former is perfectly pure, should be well compared. But even if they should yield the same ingredients, we still would not admit their chemical identity; for in analysing the Tesselite of Faroe, we actually analyse a combination of crystals with one and two axes, and therefore, it is essentially necessary to an accurate comparison of the two substances, that the *biaxal* part of the Tesselite should be analysed separately from the *uniaxal* part, the last of which has the same general optical properties as the *Apophyllite surcomposée.* Let us suppose that such a nice operation is capable of being executed by a dexterous analyst, and that the *biaxal* part is found to have the same composition as the *uniaxal* part, then we shall be forced to the conclusion, that minerals may have *different optical structures,* and yet be *chemically the same substance.* But such a conclusion, instead of being unfavourable to the optical method, tends only to prove the superior delicacy of its indications, and to increase our confidence in its results.

EDINBURGH. }
May 15. 18

PLATE II.

House of Copernicus.

Fig. 5.

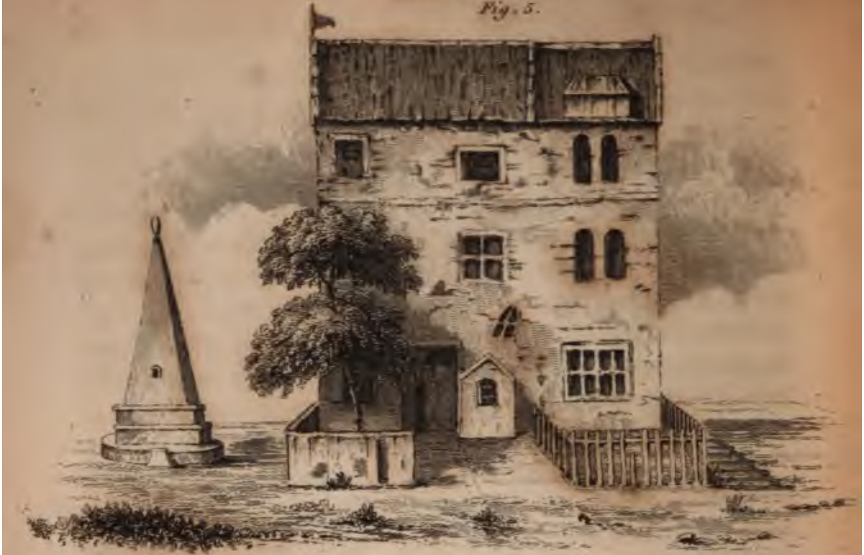


Fig. 1.

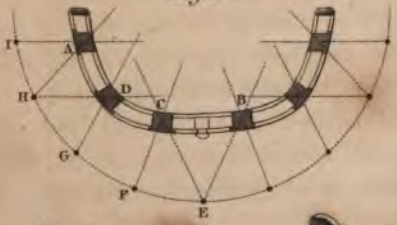


Fig. 2.

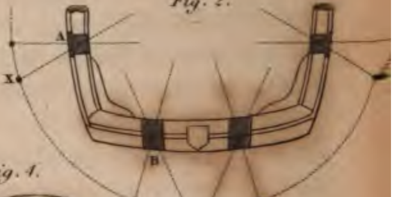


Fig. 4.

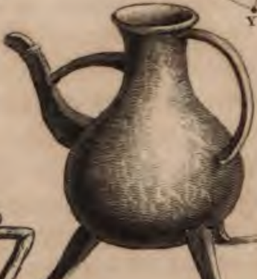


Fig. 7.

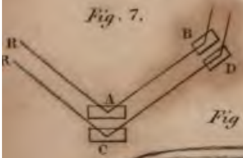


Fig. 3.

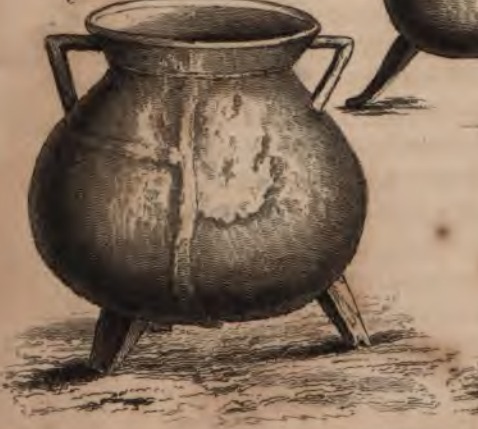
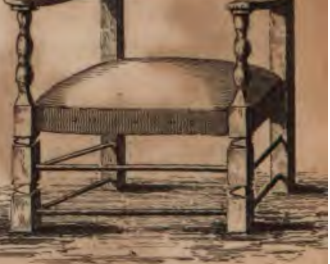


Fig. 6.
Chair of Copernicus.



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Engᵈ by W. H. Lizars

Art. III.—*Observations on Sir* Robert Sepping's *Plan for the Circular Sterns of Ships of War.* By George Harvey, Esq. Member of the London Astronomical Society.

FEW naval architects, of any age or country, have been more singularly fortunate in the original conception of important and useful designs, than Sir Robert Seppings; and the great success which has attended the practical application of his plans, has commanded a respect for his name, which, there can be but little doubt, will increase in proportion as their excellence and utility shall be better comprehended and known.

But there is one of the designs lately brought forward by this distinguished individual, which has been questioned with a keenness and severity of a very uncommon kind; and, it may not be unfairly added, has been opposed by prejudices of no ordinary stamp. From the infancy of naval architecture, up to the present moment, no branch of it has undergone such varied discussion,—been canvassed and examined with so much interest and zeal,—and produced so many singular, and, in many instances, unmeaning comments, and contrary opinions, as the change which this eminent surveyor is desirous of introducing into our ships of war, by converting the square into a circular stern.

It is one of the rare merits of Sir Robert Seppings, that all his plans are of a useful and practical kind; that being founded originally on the best experience, and undergoing, in every instance of their application, a strict and rigorous inquiry, they have in most cases been productive of great immediate benefit to the public, and consequently entitle their author to the highest honours which a great and powerful nation can bestow.

It is, however, sometimes the fate of the most important and beneficial improvements, on their first introduction, to be questioned with unusual harshness and severity; and the spirit of this opposition is, in general, in proportion to the degree in which the proposed innovation happens to depart from long established usage. The whole history of science is filled with lamentable proofs of this frailty of our nature; and we need not even go beyond the borders of the present century, to meet with many, very many, proofs of the baneful influence of those active and unfortunate prejudices. The Safety-Lamp, the most inestimable of the discoveries of the illustrious Davy, was destined, on its first introduction, to meet with an opposition of this kind; but time, which always renders more conspicuous the triumphs of genius, has placed it on the firmest and best foundation, and shown that it is "rich in blessings to mankind." That consequences equally satisfactory must ultimately result from the plan of the Circular Sterns, there can be but little doubt. The opposition which it has met with, has only served to quicken inquiry; and now that its ingenious inventor has brought the subject before the public *, in a shape which will enable every one interested in the inquiry to examine it for himself, the merits of the question must be fairly and impartially considered; and those objections which imperfect practical information may have urged, or that opposition which seems to have owed its origin to certain preconceived notions of beauty of external form, will unquestionably vanish before the conclusions drawn from a sound and enlarged experience.

Circular sterns, when contrasted with those of a square form, may be contemplated under two points of view. In the *first* place, We may inquire into the strength peculiar to each form, considered as a system of mechanical forces; and, *secondly*, The means which each affords for carrying into effect those objects for which a ship of war was primarily constructed, namely, attack and defence.

In the mechanical construction of a ship, every part of its structure ought to possess a proper degree of strength, no one part possessing, if such an expression may be made use of, *more* strength than is absolutely necessary, nor any part *less* strength than the nature and office of that particular part is destined to maintain. And it is in the due adjustment of the several parts which constitute the frame of a ship, considered as a system of mechanical forces, that the science and judgment of the naval architect find so wide a field for the exercise of his powers.

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* Sir Robert Se [redacted] blished a Letter on the subject of Circular Sterns, addressed to

Where a general similarity of construction prevails, it is impossible to derive any information from comparison. No advantage, for example, could be derived from comparing the square stern of one vessel with the square stern of another, supposing equal skill to have been employed in their construction. But we may arrive at some satisfactory information, by contrasting the strength and firmness of structures of *different* forms,—the strength of the stern of a ship, for example, with that of the stem. It may indeed be urged, in opposition to such a comparison, that, independent of the dissimilarity of form which at present actually exists between the stem and the stern, the duties which they are destined respectively to perform are so very opposite to each other, that nothing satisfactory could be hoped for from the comparison. The dissimilarity of form, and the difference in the respective offices of the parts just alluded to, will be immediately admitted. But if it should appear on examination, and by an appeal to authentic documents, that a weakness in the stern is *much more* common than in the bow, then will both these objections be fairly disposed of, and a superiority in the formation of the bow over that of the stern will be the necessary consequence.

To enable us to institute this comparison in the most satisfactory and perfect manner, Sir Robert Seppings, in the first Appendix to his able Letter, has furnished above 120 examples of ships of different classes, the sterns of which have been made the subject of *frequent* and *strong* complaint by their respective commanders. To increase the value and importance of these documents, it is worthy of observation, that they have not been collected from any very limited portion of time, or when any particular feeling in favour of a *change* of form might have existed in the navy, but during a period of nearly a quarter of a century, and through the trying services of a long and active war, and when the attention of every naval officer was necessarily directed to the *actual* state of the ship he commanded. These evidences, also, it may be farther observed, in favour of the weakness of the square stern, have been selected from a multitude of other official reports of the same kind, drawn up by able and experienced officers, placed in circumstances of a very varied and difficult nature, and with no other object in view than the good of that service to which they have so honourably devoted their lives *.

To group together facts, it has been observed, which have some important qualities common to them all, is the main scope and business of philosophy. Now, the examples contained in the Appendix alluded to, (of which those in the preceding Note may be regarded as a specimen), affords a most striking instance of the value and importance of this remark. Every document in the table bears a decided testimony to the *uniform weakness* of the stern. This is the common point or focus to which all the remarks tend; and therefore, it may be added, without fear of contradiction, that the formation of the stem has decided advantages over the present formation of the stern.

But the change which Sir Robert Seppings contemplates, and which he has actually applied to several ships, is to communicate to the stern the strength and firmness of the bow, and to continue the diagonal system of building which he has lately

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* For the information of those who may not have seen Sir Robert Seppings' Letter, it may be observed, that the Appendix here alluded to, consists of extracts from the official reports of the officers who have commanded the different ships alluded to in it. The ships amount to about 120, namely, 60 of the line, and about the same number of frigates. The following is a specimen of the Appendix:

Rate.	Guns.	Ships' Names.	Captains' Names.	Date.	Nature of Defects.
3	74	Bellona,	T. B. Thompson,	Aug. 1799.	The poop works very much, so as to work the foremost bulk-head partly down.
3	74	Minotaur,	Mansfield,	Oct. 1806.	The poop is in a very weak state, wants additional security; and when the ship rolls, the whole body of the poop goes over from side to side.
2	98	London,	Western,	May 1809.	The ends of the stern-timbers have forsook their seats, or scores, on the transom, on the larboard side. The stern shakes or trembles much when in light winds, but much more so when in heavy seas.
3	74	Ganges,	Dundas,	Mar. 1811.	The stern frame and post work so much that the oakum works out, and the nails are drawn in the end of the deck, which occasions leaks in the bread-room.
4	50	Adamant,	Ho[illegible]	[illegible]01.	The stern-frame has fallen so, that the stern-timbers are nearly out of their steps on the wing transom.

introduced round the stern, in order to make the strength of the fabric uniform and complete. It is now universally admitted, that the diagonal system has communicated great strength to every part to which it has been hitherto applied; and there seems no good reason why the same increase of strength should not be communicated to a part so notoriously weak as the present square stern, particularly, when, by doing so, not only the mechanical frame of the ship is materially strengthened, but its means of defence also very much increased. Sir Robert, at p. 6. of his Letter, very properly remarks, " that circular sterns are formed, and in all respects timbered and secured in the same manner as the bow," and that " the strength of the circular stern is *equal* to that of the bow," and consequently equally well adapted to withstand the shock of the sea. No authority can be more convincing and satisfactory than this, reposing, as it unquestionably does, on the soundest experience, and supported collaterally by so many strong and undeniable truths.

The next point of view in which this important subject may be contemplated, is the consideration of the means which each form of the stern affords for attack and defence.

In the *first* place, the same objections may be urged against the defence of a square stern, as is known to attach to a redoubt of a square form. " Redoubts," says Malorti de Martemont, in his Theory of Field Fortification, " when they are not flanked by some other fire, have two essential defects; the first is, that their saliants are unprotected, which cannot be remedied, but by adapting to those saliants a few teeth of *cremaillère*, or when the ground and every other circumstance will allow it, by directing the saliants towards some inaccessible points, or by placing in front of them, when possible, some artificial obstacles." " But," continues Malorti, " *Circular* redoubts have not that defect, as their fire which has no fixed direction, may *incessantly vary*, and *spread* itself on *every point* of the ground that surrounds them;" and " the defence which they present is *uniform on every part of the circumference*." Now, the defects which this able writer attributes to square redoubts, hold in all their force against the defence of the square stern; while the reasons he gives in favour of redoubts of a circular form, apply most favourably to ships with round sterns. If we may be allowed, by way of illustration, to borrow a few terms from the practice of field fortification, to apply to the mode of defending a ship of war, we may without impropriety say, the " saliants" of the *square stern* are unprotected; that we cannot apply to those " saliants" a few teeth of cremaillère;" much less direct the " saliants" towards " inaccessible points," or erect in front of them " artificial obstacles." But that the fire of the circular stern *is* without a " fixed direction," because it will " spread" itself over every point of the ocean that surrounds it, and moreover, that its defence will be found " uniform" in every part of the circumference. It is true, that the defence of the stern only includes the form of a semicircle, while the defence of the redoubt here alluded to, embraces the whole range of its circumference; still the reasoning holds good for the latter figure, as well as for the perfect circle, because the chord of the semicircle, in the case of the circular stern, requires *no* defence.

To illustrate these remarks, by means of a diagram, we may refer to Plate II. Fig. 1. being a plan of the gun-deck of a ship of 84 guns, with a round stern. In the first place, the right aft and quarter guns at B, C, D, can be brought at the same time, so as to defend the arc E, F, G; and if the arc GH requires defence, the gun at D may be placed at different angles, so as to command it; and it may even be assisted by the gun at the after broadside port A, this gun commanding the arc IH. The same remark will apply to the other quarter of the ship, that so the *whole* stern may be powerfully defended at all points by the guns on the different decks,—a defence which a ship with a square stern would be incapable of making.

And how necessary such a mode of defence may be at times, has been most clearly and forcibly shewn by Sir Robert Seppings in several instances. Among those mentioned in his Letter, we may allude to the attack made on the Gibraltar, Northumberland, Terrible, and Powerful, by gun-boats in the Bay of Gibraltar; and also when the Minotaur and Dictator passed the Belt, by gun-boats raking them in a calm. In the retreat also of the squadron of Admiral Cornwallis before the French fleet, they had no [illegible] firing but *right aft;* and in order to accomplish this, [illegible] rves " they were mutilated to such a degree, to [illegible] apply their guns, that a refit of no small extent was necessary, before they could be considered again fit for service."

To contrast more clearly the defence of a circular stern with one of a square form, we may refer to Fig. 2. where it appears, that although the guns at the ports A and B may be trained to the greatest angles which the situations of the ports will admit of; yet the arc XY, comprising from 40° to 50°, will be entirely without defence. A similar defect, it is apparent, must exist on the other quarter of the ship. In fact, the only part of the square stern that can be well defended, is the arc YZ, comprising about 40°, the remaining part being entirely without defence. In the former figure, it was proved that no part of the semicircular sweep round the stern was undefended.

"In the event of future wars," observes Sir Robert, "an alteration in the form of the stern of our ships of war would in all probability be absolutely necessary, by which the guns may be worked with greater effect and facility, in consequence of the introduction of steam-vessels: and that America is firmly convinced that a system of attack, by this description of vessels, is not only practicable, but that it will also be destructive in its operations, is not to be doubted. Indeed," continues Sir R. "I have been told, from good authority, that they have lately well manned one of their frigates, given the command of her to a good officer, and directed an experiment to be tried, if a vessel propelled by steam could not, under any circumstances, lay on the *quarter* of the ship she attacked, and the result was completely in favour of the steam-vessel." If we inquire into the cause of this failure, we shall undoubtedly find, that the frigate was incapable of defending her *quarter*, owing to the *square form* of her stern,—a circumstance which would not have taken place, if she had possessed one of a circular form.

In case of an attack of the kind just alluded to, Sir Robert Seppings' plan gives a superiority, not only on account of the additional number of guns which the ship is enabled to bring into action, and the sweep of the ocean which those guns are able to command, but also an advantage of another kind, arising from the diminution of the distance of that point from the ship where the shot from the two after guns cross each other, when the guns are trained to their greatest angle,—a point, it is pre sumed, of no small importance in case of an attack from a steam-vessel. In an 84 gun ship with a square stern, this point is distant from the stern, on the level of the gun-deck, about 18½ feet, and from the upper deck about 16 feet; whereas in the circular stern, the same point on the gun-deck is distant only 12 feet, and from the upper deck but 13. In the case of a 60 gun frigate, the distance of a similar point from the square stern is nearly 16 feet, while in the circular stern it is only 9. These united considerations tend unquestionably to prove, that the circular stern is the form best capable of defence.

The sentiments of so distinguished a man as Dupin, on a question of this nature, are entitled to the highest consideration; and it is most pleasing and satisfactory to peruse the candid and sensible remarks he has made on the subject of round sterns, in his different valuable works. "It is in the sterns of their ships," says he, "that the English at this time carry into effect a great improvement. For the future (it is to be wished that the liberal views of Dupin were completely realised in this point) the sterns of their ships are to be circular, composed of timbers, and planked up in the same manner as the sides of the ships. They are to be pierced with ports, shut in with solid port-lids (below). The ships of three decks, for example, present four vertical ranges of four ports each, to fight when in retreat, which, in proportion to the breadth of the ship, present a force equal to that of ships the best armed in their sides. In the (square) stern, the many glazed windows, and the munnions of fir are the only defence; notwithstanding a shot which comes into a ship from aft forward, or from forward aft, causes ten times the destruction which a shot does which comes in at one side and passes out at the other. Act contrary, then," continues this enlightened author, "to what has hitherto been the practice, and, if possible, make the means of defence of the part *most* exposed ten times as great as it now is. Reason, at least, demands that this should be done.

"I ardently wish to see these improvements introduced into *our* ships. I have constantly repeated, since my first journey into England un [illegible] day, every argument, every observation, every exper [illegible] have appeared to me to carry conviction to th [illegible] ices and objections, which it is impossible to overcome except by the aid of time, have presented themselves; but over these time will necessarily triumph."

Dupin has alluded to *prejudices;* and unfortunately objections, having no better foundation, *have existed*, and *do still exist* in this country, against this most important plan. Among these prejudices, one may be mentioned, which has given rise to objections respecting the want of beauty in the external form of the circular stern. It will not be contended, but that, according to the mode in which the eye has been hitherto *educated*, the appearance of the square stern is rather more beautiful than that of the circular form. But the influence of education, it has often been remarked, is omnipotent, and *time* will do much for the *appearance* of the new plan. Is it not, however, trifling with a subject of so very important a nature, to sacrifice *utility* to appearance? Ought the sound principles of a genuine mechanical construction to be abandoned, because the *eye* has been wedded to a particular form? Ought not the primary objects for which a ship of war was constructed, to be consulted, rather than any ideal notions relating to a subject so very abstract and uncertain as that of beauty? A ship of war should be contemplated at all times,—whether in an actual state of warfare, or during a period of tranquillity and peace,—as a floating castle, possessing all the essential elements of durability and force, and capable of acting under all situations, whether of an offensive kind, with equal power, certainty and success. This, it must be admitted, is the idea which ought ever to be associated with a British man-of-war, destined, as it is, to act under circumstances so very varied and uncertain. Objections relative to appearances should cease, and even convenience ought to be surrendered, if necessary, when so many advantages are likely to be obtained. "Ornament and decorations can only be perfectly beautiful," as Dupin ingeniously observes, "when the appearance of force commands respect from the enemy." "What should we say," continues this judicious observer, "of a military engineer, if he were to substitute for thick walls, and the massive gates of curtains, *glass-doors*, to please the fancy of the officers of the place? "Yet," says he, when referring to the square sterns, "*here are* the glass-doors which we open in the curtains of our floating fortresses."

In concluding these remarks, therefore, it may be observed, that Sir Robert Seppings, in his letter to Lord Melville, has most unquestionably demonstrated two essential and important principles, namely, 1*st*, That a ship's stern, when constructed of a square form, is weak and feeble, when compared with the present general structure of the vessel; and, 2*dly*, That vessels with sterns of the same form, are incapable of making a strong and vigorous defence, when attacked by a powerful force in that part; but that vessels with circular sterns possess qualities and powers precisely the reverse, exhibiting strength where weakness is invariably found in the old form, supplying all the defects attendant on that mode of construction, giving strength to the entire mechanical frame of the ship, and affording increased security and power to the gallant men who navigate and defend her.

PLYMOUTH,
6th April 1822.

ART. IV.—*Account of Professor Reinwardt's Journey through the Preanger Regencies in Java*[*].

THE districts which have been explored by Professor Reinwardt, for the purpose of investigating the native condition of the country, are Tjifondarie, Banjaran, Tjiparay, Manabaya, Timanganten, and the Regency of Limbangan. The principal mountains which he ascended, extending themselves south-east from Mount Gedè, are the Patacka, the Tombak-Pacyong, the Tiloc, the Malabar, the Sumbong, the Gadja, the Gainang-Gaintoer, and the Talaja-Budar.

The district of Tjifondarie, on account of its more elevated situation, is distinguished above all others by a very temperate climate, and by great fertility, as well as by the beautiful prospects, which are enriched by a sight of the loftier mountains towards the south and south-west of that country, and these indeed, were among the most interesting scenes of his investigation. The extensive mountain Patacka, which is the most remarkable of the range, elevates itself next to Mount Gedè, above most of the other peaks of the adjoining ones; the height of this is about 7400 English feet above the surface of the sea. Near the summit are still seen many proofs of early explosions or eruptions, so that this mountain may be reckoned amongst the more ancient volcanoes of Java. On the very top of this mountain are two very wide abysses, which probably are the ancient craters of volcanoes. The one on the south side of the mountain produces a very striking spectacle. It is an extensive circular bason on the very summit of the peak, at the bottom of which, to the depth of more than 700 feet, is a great lake of sulphur. A considerable quantity of sulphur is also spread over the sides of the bason, and mixed with decomposed stones, forming a yellowish-white crust all around this abyss. The sulphureous vapours have penetrated even through the rocks of the mountain, which are originally of a black basalt, and have partly dissolved them so much into a loose white sand, that their former nature is with difficulty distinguished.

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[*] This account appeared originally in the *Batavian Courant*, from which it was translated into the *Calcutta Journal* for August 1820, p. 411.

The second abyss, which is on the north side of the top of that mountain, is also a spacious and deep bason, but its bottom is dry, and scantily overgrown with wild plants.

The particular Mountain of Patacka, as also the other elevations of the range, being more or less connected with the others to the north and west of them, compose one part of the district of Ronga, in which is also the Mountain of Tombak-Pacyong, of the elevation of 5900 feet. These are everywhere covered with thick woods, which afford an amazing variety of plants, changing in their nature at every different degree of height. The prospect from the summit of the Patacka, especially towards the south, over the district of Tjidam, extends as far as the sea, over a wilderness, and in other directions offers a variety of prospects, combining hills, woods, and an endless number of singularly shaped rocks.

From the cold atmosphere of the district of Tjifondarie, the rice fields produce a later harvest than in the other provinces; but many Indian fruit-trees cannot be reared there, as in the milder climates below. On the other hand, the nature of the many plants growing wild there, and which are suited to the temperate climate of Europe, prove that this part of the country would be as favourable to the culture of many European plants, as Europe itself. At Tjimeding, which is the chief place of that district, the medium range of the thermometer is 70° of Fahrenheit, and on the top of the Patacka, it is often as low as 40°. To the east of the Patacka, at the distance of one and a half German miles, and separated by a ridge of less lofty hills, lies the Mountain of Tiloe, (which name signifies three mountains,) composed of three distinct peaks, of which the first being the most southerly, rises to the height of more than 6000 feet. The stones of that and the surrounding mountains exhibit a greater variety than those of Mount Patacka. The most pure and unmixed basalt is found on the higher mountains only. The Tiloe is on all sides covered with thick woods; and the rocks on the surface are decomposed to a considerable depth, which is highly favourable to the formation of the soil, and supports the most exuberant vegetation.

The mountain, bearing the name Malabar, is known to be of a greater extent than the others, and is situated in the southeasterly part of the district of Banjaran. This range is composed of many connected mountains, of which the loftiest elevates itself to a height of 6100 feet above the surface of the sea; and is, on account of its situation, its products, and in many other respects, more remarkable than all the other mountains on the island. The rocks, being, from the sudden precipitations by explosion and convulsion, in many places heaped together upon each other in large irregular masses, exhibit a greater variety to the eye of the beholder, than is elsewhere to be seen. The basis of the mountain is a basalt, partly pure, and partly mixed with different other stones, sand, or dust, amongst which there is also lava, and a volcanic kind of rock, containing iron in such quantities as to produce a forcible effect upon the magnet. The Mountain of Malabar has a great portion of its length cut through by a deep valley, extending from the south to the nor [illegible] ough which runs the River Tjigeurre, which first precip [illegible] om a perpendicular cataract of some hundred feet [illegible] on with great velocity till it makes another fall between the mountains, after which it unites itself in the plains of Banjaran with the River Tjisankooi. The Mountain of Malabar, and the plain here spoken of, unite all, that great beauty and variety of scenery, and the profusion of a very rich and fertile soil can produce, to be striking and remarkable; and here, indeed, the travellers found the most ample materials for observation.

Professor Reinwardt proceeded farther on in his journey, north and east of Malabar, going through Tjipalag and Marabaya along the banks of the Tjitarum, towards the south, till he came to the place of its proper origin. The high country where that river takes its rise is a large plain, rising gently towards the south, and is open to the north only, but shut in on all the other sides by high mountains. This plain, though entirely uninhabited, and even scarcely known to the greater part of the inhabitants of Java, nevertheless exhibits one of the finest and certainly one of the most fertile parts of the island. Between its luxuriant woods are rich pastures, where the wild cattle, deer, and other grazing animals undisturbedly enjoy abundance of food. Some of these rich pastures being surrounded by forests, or partly overgrown with groups of trees, appear like extensive parks, laid out by art. In one of these plains, situated on the south-east of the Mountain of Sumbong, the water, rushing down from hills, collects itself, and forms the source of the River Tjitarum, which is afterwards augmented by other smaller streams, issuing from the mountains on all sides of the plain. The climate of that part of the country is very temperate, the medium range of the thermometer being 62° of Fahrenheit. The barometer stands at 25½ English inches, which proves the descent of the Tjitarum, from its origin down to the sea, where it discharges itself, to be 4640 English, or 4493 Rhenish feet.

The Mountain of Sumbong, if ascended from this plain, affords, at a height of about 5600 English feet, a wide prospect along that country, as well as on the west side of the Papandagang, out of which district, particularly from that side, rises a great quantity of vapour.

It did not appear to our travellers, that the Tjitarum takes its origin at the same place with the River Tjilakie, as indicated in the map of the late Mr Beitjes. The swamp is, however, on that side shut in by a rising ground.

After having returned from the plains, our travellers proceeded on their way through the woody hills, north-eastward over the mountains of Goha, Tangar, &c. and through the district of Manabaya, in order to reach from that side the other mountains of Gadja Massigrit, and the Gainaing Gointor. This trip was one of great difficulty to the party, not only from their being obliged first to make their way through an extensive desert, through thick forests, and over a continually ascending or descending ground, but also on account of the amazing quantity of loose ashes, thrown out to a great distance by the Goinaig Gointoir, which covered the ground to a considerable depth. Amongst the many remarkable objects which there presented themselves to the view of our travellers, a place on the mountain of Kismis, situated on the border of the districts Manabaya and Timanganten, deserves particular mention here. This place bears the name of Kama Karaha. Clouds of a whitish vapour rising from out of a cleft in the above named mountain, sufficiently indicate, even from a great distance, that a subterraneous fire is active there. The place itself produces such a spectacle as cannot be beheld without astonishment. A black, muddy, boiling water, mixed with lava, is thrown out in high and foaming waves with a great noise, from different wide and deep gulfs; spouting from an innumerable multitude of openings, dispersed all over the surface, the excessive heat of which makes the ground in many places quite impassable. Brimstone, a whitish salt decomposed, and various high coloured sands and stones, cover and variegate the ground. Rocks fallen down, and black burnt trunks of uprooted trees, mixed with each other, lie dispersed on the soil, while the atmosphere is filled with thick sulphureous and misty vapours. The road from that remarkable place ascending further gradually up towards the Mountain of Gadja, became continually more and more difficult to the travellers, occasioned by the increasing quantity of ashes, which every where covers the ground. The wood, through which these waters ran, scarcely presented a single tree that appeared to be still alive, merely the dead b [redacted] unks remained standing; nevertheless, on coming awful scene of vegetable destruction, the party were surprised by the sight of various young plants, and astonished at the particular manner in which they grew,—for instance, the crown of the leaves would have shot forth from out of the heart of the trunk, had the thick bark been able to protect the same against the deadly scorching caused by the heat of the lava. Wild plantain and other wild shrubs had indeed shot up from out of the deep ground through the midst of the very sand and lava that covered it. Nothing, say the travellers, can give an adequate idea of the effects left by the last eruption of the Gointoir, but a visit to the scenes themselves.

Proceeding along the south side of the mountains of Gadja and Massigit, the travellers soon arrived at the Mount of Gointoir itself. On the north side of it, near the summit, and not far from the spot where lies the crater of that remarkable volcano, they rested for a considerable time in expectation of more favourable weather and a clearer sky, which might afford them an opportunity to inspect and investigate the different objects of their search with more advantage, but the air continued to be very cold, and the hills entirely enveloped in damp clouds, which, together with the difficulty of bringing up the necessary water to such a considerable height, along a very steep road, covered with sharp stones thrown out from the crater of the volcano, obliged them to abandon their intention of a longer stay at that place. However, it appeared, that the road by which they had now ascended, though extremely difficult, was notwithstanding much easier and better than that which the year before Mr Reinwardt had chosen for the same task. The height of this volcano above the surface of the sea is more than 6000 feet.

The Talaga Badas, situated in the district of Manaradja and regency of Limbangan, also belongs to these mountains, which, on account of the violent effects of the heat coming from out of the ground, is also very remarkable. On this mountain, near its summit, is also a lake of sulphur, shut up in a spacious bason, like the lake of sulphur on the top of the Patacka. Yet the phenomena on the top of the Talagas Bodas, are, on account of the more violent effects which still take place there, much more striking than those of the other. Besides the several wells of boiling water and hot sulphureous vapours, there rises from out of an opening in the mountain on the south side of the lake, a great column of brimstone vapours; the steam being driven up from the deep abyss with great violence, and striking against the rocks above, is heard at a considerable distance. From other narrow openings a boiling sulphur is seen bubbling up, which afterwards spreads itself all around over the rocks. The lake, forming an oval, has a diameter of from 1500 to 2000 Rhenish feet; the water therein exhibits itself as purely and entirely white, from the incrustation at the bottom; the sides of the mountain in many places glitter from the transparency of these brimstone incrustations, while black masses of basalt and burnt trunks of trees scattered around, present a striking contrast, and shew also what great changes these mountains have already undergone, and to what future changes they remain still exposed. The vapours that display themselves here, appear to possess more dangerous qualities than those at the other places; besides which, from the steepness of the slope down which the overflowings of the springs run, on the east side of the mountain, the brimstone and wells of water have already caused great devastation in breaking up the soil and rocks. One finds there always many carcases of wild animals which have evidently been killed, either from the drinking of the muddy water, or from the inhalation of the sulphureous vapours. Even the stronger beasts, such as the tiger and kidang, are not safe. It is a singular fact, that of these killed animals, the bones very soon disappear, whilst the softer parts and the skin remain for a long time in existence.

This journey of Professor Reinwardt through the above-named districts, has gained him the knowledge of a great number of natural productions. The vegetable kingdom in particular has every where presented to him an uncommonly great diversity of plants, which in every variety of shape and colour, continue to grow, even on the tops of the highest mountains. The greater part of the wild animals there frequent more the less elevated hills, where the woods yield them more nourishing food. Yet the rhinoceros, which is found every where in these elevated re[illegible] ascends with an astonishing swiftness, even to the high[illegible] mountains; it is on that account that this animal [illegible] be found, and that it was frequently pursued in vain, till at length the party were successful enough to overtake a couple of them. One of these beasts was shot near the mountain of Papandayang, and the other close to Mount Talaga Bodas. Both are secured to increase the collection of natural objects, which this successful journey has procured for the Museum, already so extensively enriched by the enterprise and scientific ardour of the Professor and his companions.

ART. V.—*History of the Invention of Pendulum Clocks by* CHRISTIAN *HUYGENS.* By J. H. VAN SWINDEN, Councillor of State, Professor of Philosophy at Amsterdam, &c. (Concluded from Vol. VI. p. 213.)

2. BUT, it has been said that Huygens got intelligence through his father and Deodati of Galileo's labours. It is true, Deodati wrote to Constantine Huygens, father of the philosopher, and Secretary to Prince Frederic Henry, pending the above negociations with the States-General; for his letters are of the year 1637, but they contain only the same intelligence which has already been given (See Galilei Op. iii. p. 432.); and Huygens could learn nothing more than what shortly after appeared in the *Dialogi de motu*, except perhaps his notion of the pin and wheel, to indicate the number of oscillations without the trouble of counting them,—a thing as totally different from the principle adopted by Huygens to regulate clocks by means of the pendulum, as any of the attempts of Hook and Hevelius, who, however, never claimed the discovery of its application to clocks. And as to a clock or model having been sent over to Holland, we only learn, from a letter of Deodati of the same year, that if the States desired, Galileo would send what he had promised, to-wit, " a very perfect telescope, with the method he had found of using them on board vessels, whilst in an agitated sea; the observations about the motions of the satellites of Jupiter, and the construction (*fabricam*) of the very accurate *horologium* invented by him; and that he would explain all his inmost thoughts on this subject in words and writing." This

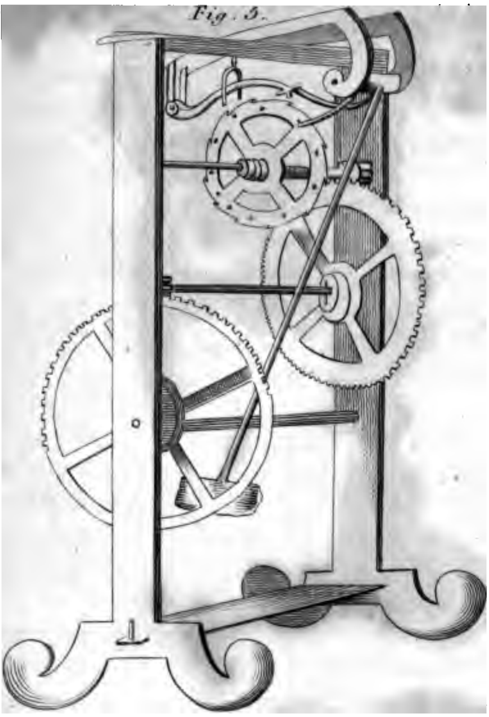
manifestly refers to the description already given above. The mistakes on this subject have arisen probably from a prevailing notion, that Hortensius, the Amsterdam professor, had been actually dispatched to Italy. This is confidently asserted by Brucker, in his *Historia Philosophiæ*, vol. v. p. 673, and has been copied in Bailly's *Hist. de l'Astron. moderne*, and Montucla's *Hist. des Mathematiques*. The fact is, that Hortensius never departed on this voyage, though great preparations had been made, and the States had granted 2000 guilders for it; it was first delayed, and then prevented by his death. Of the truth of this any one may be convinced, who will be at the trouble of perusing the letters of Vossius and Grotius, written, it must be remembered, about twenty years before Huygens claimed any invention, or before any controversy on the subject could arise.

3. Of more consequence appears the direct assertion in the Acts of the Academy *del Cimento*, which we quoted before, that the idea of conjoining the pendulum with the clock had been first of all conceived by Galileo, and actually put in practice by his son Vincenzio Galilei in 1649. I have already, I believe, abundantly shown, that, in his communication to the States, Galileo had suggested nothing that can with propriety be termed a *clock furnished with a pendulum*, the only contrivance to which Huygens lays claim. His last letter is of the 30th December 1639, but he may, before his death, in 1641, have made the discovery, or his son, on the suggestion of his father, may have found it. From what I am enabled to produce on this head, it appears to me unquestionable that Vincenzio did accomplish, or endeavour to accomplish something.

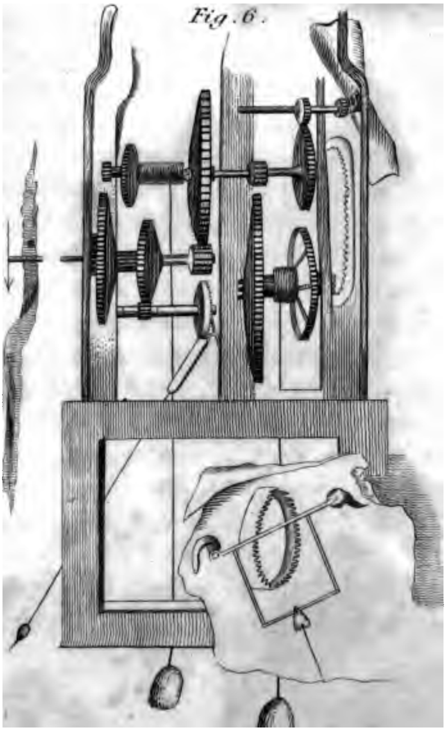
We need not go back to the period of 1636, to which the supposed invention of pendulum-clocks was referred by Prince Leopold de Medicis, in the same letter to Bouillau, the friend of Huygens, of which an extract was given in the earlier part of this narrative, except to remark once more how egregiously Galileo's communications to the States-General had been mistaken. Indeed, so little have these been looked into by the writers who profess to found their claims upon them, that Brenna, in his life of [redacted] (in Fabroni's collection of *Vitæ Italorum excellentiu* very letter which I have extracted under the fi 's of it, " De fabrica atque usu

PLATE I.

ROUGH SKETCH of a DESIGN by GALILEO.



ORIGINAL REPRESENTATION of a CLOCK at FLORENCE.



Horologii pendulo muniti, quod ipse excogitaverat, admodum luculenter ac diserte loquitur," *a clock furnished with a pendulum*, when the letter contains in reality the description (in Galileo's own words) of a free pendulum, a vibrating sector. I shall only quote one passage more from the above mentioned letter to Bouillau, in which the Prince says, "Having seen a model constructed by Galileo, differing somewhat from that of Huygens in the disposition of the wheels, (in parte deverso circa la constitutione *della ruote*)," (*Leyden* MSS.); because this (which probably regards Galileo's son as much as himself, who was blind after 1639,) agrees with what had been written to Huygens from Rome in 1660, about a rough sketch of a clock attempted by Galileo, and also with the account of the academicians *del Cimento*, and is besides confirmed in a striking manner by Frisi, in his *Elogio del Galilei*, Milan, 1775, where he mentions Leopold's opinion, that Galileo had put nothing into practice equal to the invention of Huygens, "as might be seen from the little that had been *contrived* or *sketched out* by his son." "And truly," he adds himself, "the machine of 1649, was nothing but slightly sketched (*non era che un poco d'abbozzo*)." He seems to think that the clock represented in the experiments of the academicians was made after this model: this is not probable, as they are of a much later date.

After mentioning the concurring testimony of these writers, that something had been made or sketched by Galileo or his son, I have now the satisfaction of presenting to the class a *fac simile* of the drawing, representing the contrivance in question. (See Plate I. Fig. 5. and the Notice at the end of the article). It was found by me among the papers relating to Huygens, in the Leyden Library: There is written upon it, in Huygen's own hand, *Missa a Ser*°. *Principe Leopoldo ad Bullialdum ab illo ad me: R*. 15. *gan*. 1660, *cum descriptio mei Horologii edita fuisset, A*°. 1658*;* and in another hand, which I recognise for that of Bouillau, these words, *Horloge commencé par Galileo Galilei;* and on the reverse by the same, *à Monsieur Monsieur Christian Huygens de Zulichem à la Haye.* Among the letters of Bouillau, there is a corresponding one of the 9th January 1660: "I send you the figure of the pendulum-clock begun by Galileo, as it was sent to me from Florence; and another of the 29th February. You may keep the drawings: I kept the copies sent from Florence; I had yours made after them." (*Leyden* MSS.)

It is manifest, from the mere inspection of the figure, that the drawing was made in a very coarse manner: I suppose, therefore, that the lowermost wheel had teeth on its whole circumference, though but one-half is represented, and likewise that the next following or middle wheel was divided equally by teeth every where, though a part here consists of merely a few hasty strokes; probably to meet the convenience of the artist who drew them. The axis to which the pendulum is affixed, and which moved with it, has a short bent lever, reaching to a number of studs or pins, projecting from the side of a sort of ratchet wheel, the teeth or incisions of which are equal in number to the pins. Above this wheel appears a crooked lever, fixed at one end, on which it turns, and seeming to rest with the other extremity against the axis or end of the pendulum, somewhat like a spring: it is also furnished with a projection, in the form of a cross, to enable it, as it would seem, to catch in its descent the teeth of the wheel. Farther, there appears neither spring-barrel nor cylinder for a weight. The only indication that can be found of its having been moved by a weight, is perhaps to be traced in the axis of the uppermost wheel, a part of which has an indistinct appearance of grooves cut into it, as if a rope or chain were to be wound round it.

As to the action of this machine, I can conceive the short bent lever to strike against one of the projecting pins, impelling it either in the one direction or the other, whilst the pendulum is swinging to one side; but I do not so easily perceive the way in which, on swinging backwards, it should fall in with the next following pin; and supposing this to be possible, then it is inconceivable to me how the loss suffered by the pendulum in its passage through the air and by friction, could be exactly compensated, as happens in the clock of Huygens, by the reciprocal action of the crown-wheel on the pallets, a contrivance of great ingenuity, which was not at first clearly understood by all. Whether Vincenzio Galilei greatly improved upon the design of his father, is [illegible] for it is of his attempt in 1649, that Prince Leopold [illegible] ak, when they call it a sketch (*abozzo*), and a [illegible] *poco d'abozzo*). But, however this may be, the above mentioned dates are sufficient to show that Huygens had no knowledge of it when he published his description. In his *Anecdota*, written by himself, he only says, "Post nostrum libellum in Italiam demissum figuras per Bullialdum a Cardinali Mediceo (Principe Leopoldo) missas; quarum Galilei alteram; sed difficili machinatione, ut non mirum non successisse." (*Leyden* MSS.)

4. We next come to Becker's narrative of his having, about the year 1677, met at Augsburg, Trefler, watchmaker of the Grand Duke of Tuscany, at the period we are considering, who told him he had been the first who made a pendulum-clock (*horologium pendulum*), at Florence, in the spirit (*instinctu*) of Galileo Galilei, of which a specimen was sent to Holland. A remark here naturally suggests itself, why no mention is made of his son Vincenzio, who is admitted by all the Italian writers either to have actually directed Trefler in the construction of the work attributed to him, or at least to have a prior claim to an attempt of the same nature. It might lead us to suspect, that the whole statement refers to the much earlier period of 1636, and alludes to the single pendulum offered by Galileo to the States-General, and called by him, as we have already seen, *oruolo*, of which Trefler, being a workman, might have made the first; and this would reconcile it to what is added of its having been made in Holland, as the only communication on the subject was made at that time.

But I shall not dwell on this conjecture, as something was certainly made afterwards by this Trefler, though the date and history of its construction are involved in great perplexity. That a clock with a pendulum, bearing the name of Trefler as maker, was produced, we have not only the authority of Frisi, (*Elogio*, p. 129.) but the testimony of Perelli, Professor of Mathematics at Pisa, who gave a description of it (Tiraboschi, p. 157.), which, however, I have unfortunately not been able to procure. The clock was said still to exist at Pisa in 1774, and may possibly have been preserved to this time. The Italian writers differ very much as to its origin. Perelli and Brenna (*Vita*, p. 77.) think, that it is the very clock devised by Vincenzio Galilei, on the suggestion of his father, and executed under his direction, by Trefler, in 1649. Others, however, maintain, that Galileo had, indeed, in 1641, proposed the application of pendulums to clocks, that his son had endeavoured, with the assistance of one Dominico Balestri of Florence, to put it into practice; but that his death (which happened in 1649, eight years after his father's) had prevented him from seeing it executed, and that only several years afterwards Trefler had accomplished it, "though somewhat in a different way from what he had intended." (Tiraboschi, p. 155.) The accounts which these writers have left us of the clock in question, go a great way to strengthen a suspicion, that it was either wholly made after the construction of Huygens, or altered, in so far as respects the pendulum. Frisi says in as many words, that except the spring-barrel and fusee, the disposition of the wheels was exactly the same as in the clocks made by Huygens; it had likewise the bent plates or checks, which, whether cycloidal or not, are unquestionably an invention of Huygens. Frisi allows the work to be his, and Perelli admits the curved plates to be an addition of a later date, and Brenna, after acknowledging the same, seems very much puzzled how to account for the rest. It is quite possible that an existing clock should have been altered by Trefler, and a pendulum adapted to it after the manner of Huygens; many examples occur of the same, as we shall presently see. But I must first remark on the improbability, that if Trefler had really made a pendulum-clock, and a good one, on principles of construction discovered either by himself or the Galileos before Huygens, these clocks should have been so little known in Italy, at Rome, even at Florence itself, as they appear to have been at the period of Huygen's work becoming known in those parts of Europe. The letters among the Leyden MSS. leave no doubt on that head. We find from Bouillau's letters, that it was first sent to Florence in the autumn of 1658; on the other hand, we learn, from the words of Prince Leopold, communicated by that gentleman to Huygens, half a year later, and which have already been given, that after three years' attention to the subject, an artist had made one, which he *hoped would succeed.* Now, we may ask, why this doubtful language, and why [illegible] ttempts, of the issue of which they confessed themsel[illegible] f Trefler had already accomplished the constr[illegible] proved to be good? There can scarcely be a stronger proof, that whatever might have been the attempts of the Florentines, they had till this time very little succeeded. Yet Huygens's book and specimens of his clocks had been sent every where, and had been known for more than a year; and at Rome a workman even excited surprise by it as an unheard of invention, and gave it out for his own!

All this seems to me to confirm in a striking manner the priority of our countryman's claim to the perfect adaptation of the pendulum to regulate clocks, and to corroborate the suspicion I stated before, that the specimens of Florentine clocks, to which so much importance is attached, were in reality made after the knowledge of his construction had gone abroad; and it must be remembered, that it was in 1657 that he already made his clocks, though the description was published a year later. Even the Italian writers, who contend for the fame of the Galileos, admit that Huygens first of all brought their attempts to perfection. (See Brenna *Vita*, p. 80.; and Tiraboschi, p. 157.) Now, it is quite natural, that, as soon as it was understood that he had discovered a way of applying the pendulum to clocks, even though his method were not precisely known, many would try to make a similar attempt at such a contrivance, at a time when a general wish existed among people conversant with the subject for an improvement of the pendulum. And this was most likely to happen at Florence, where the pendulum had been originally proposed as a measure of time, and where interest in the subject had been kept alive through the remembrance of Galileo, and the attempts of subsequent artists. The substitution of the pendulum in existing clocks was not a matter of so much difficulty, when once the mode of its action was understood; and it soon became a general practice to take out the old balances, and place pendulums instead of them.

Nor is this, in reference to the Florentines, a conjecture unsupported by any kind of presumptive evidence. From what I am now going to state, it appears that something of the nature described was actually taking place in Florence about the time that Huygens published his description. I have now to present the class with another *fac simile* of a drawing found among the manuscripts of Huygens, which his friend Bouillau sent him a few days after the former, with the superscription, *Figure de l'Horloge à Pendule qui est à Florence dans le vieil Palais de Medicis,* to which Huygens added with his own hand as on the former: *Missum a Ser^o. Pr. Leopoldo ad Ism. Bullialdum, Ao.* 1660; *cum descriptio Horologii mei edita fuisset, Ao.* 1658. This representation of a public clock on the old Palace of the Medicis, which is of itself sufficiently interesting, when conjoined with the same gentleman's correspondence, bears immediately on the subject under consideration. In the beginning of 1659, he promises to send a description of the clock which was then either undertaken or undergoing repairs by order of the Grand Duke, *(auquel M. le Grand Duc fait travailler).* The following year, when the description was sent, we find allusion to it made in several letters; the words are nearly decisive; in one, the clock which the Grand Duke had *got up, (fait accommoder);* in another, which he had *had adjusted, (fait adjuster);* in a third, which he had *had repaired with the pendulum, (raccommoder avec le pendule, Leyden MS.)* The representation itself, Plate I. Fig. 6. (which I have sometimes suspected to be one of Vincenzio Galileo's old designs perhaps somewhat altered), shows an escapement with a single pallet, as appears in the separate figure of it below, and where it is seen in front. The pendulum appears to be very light, and to describe very great arches. It looks like an imperfect contrivance, and I am in doubts whether it could answer the purpose. The remainder relates to the motion of the hands and the striking.

I shall add as a surprising fact, that Viviani, who wrote the life of Galileo, and set a high value on his reputation, speaks of him only as the inventor of the pendulum, a thing never called in question, but nowhere of the pendulum clock; and this will appear the more surprising, when it is recollected, that Viviani has been cited among the authorities to prove Galileo's claims to the invention. It is true, Tiraboschi, p. 155. afterwards produced a letter said to be written by him in 1673, in which he speaks of this latter claim as a just pretension, *(justa pretensione);* but, on the other hand, we know that Viviani lived under the same roof with Galileo the three last years of his life; that he worked w [illegible] : that the correspondence with the States-General wa [illegible] ng that time; that he knew of it, and has actuall [illegible] *Vita,* p. 78.); but without a word of pendulum clocks having been offered to be sent, or been constructed, or even attempted to be constructed;—circumstances which most evidently prove that Viviani did not then consider Galileo as the inventor of pendulum clocks, at least of such clocks as were actually made, and had been proved to answer the purpose for which they were intended.

5. The last circumstance to be considered is the story of Becher's having met with a person who pretended having seen at Prague a pendulum clock made by Justus Borgen, an artist in the service of Rudolph the Second, and under the reign of that Emperor, that is between 1576 and 1612. I suppose Becher's information to have been correct. Then it only follows, that at Prague a clock was seen, bearing the name of Borgen as maker, and having a pendulum. Nor is there any thing wonderful in this; it might be seen in many places, for as the pendulum came to be substituted for the balances in all clocks, public and private, the rest of the work would remain with the maker's name and date, if it had any, unaltered. Thus the clock on the top of the *Stadhouse* (now the Palace) of this city (Amsterdam) had, at the time of Becher's visit to Holland, a perpendicular balance, though, for more than a century, the same work is regulated by means of a very long pendulum. It is even possible that these perpendicular balances, which seem not to have been so common as the horizontal ones (Becher says, having seen them on a large scale *only* here), might be mistaken for pendulums by people not much acquainted with the details of the clockmaker's art. At any rate, if Borgen really constructed the clock such as it was afterwards seen, he must be held the discoverer not merely of the application of the pendulum to clocks, but of the pendulum itself, as a means capable of measuring time; for before 1612 Galileo had published nothing on this subject, nor were pendulums then used by astronomers for that purpose. We know Borgen, or Byrge as he is called by others, to have been an eminent maker of astronomical instruments, and Berthoud, (*Hist. de la Mesure du Temps*, tom. i. p. 37.), considers it as not improbable that he might have made these discoveries; he says, "Becher is not the only writer who attributes the application of the pendulum to Byrge." I have not been able, with the utmost pains, to discover who these other writers are, and

as he does not mention one, I must consider the assertion as unsupported by any proof. Notwithstanding this, Mr Bode had lately asserted, as an indubitable fact (*Jahrbuch* for 1816), that Byrgius had been intent on the application of the pendulum as early as 1660. So easily are errors copied when once hastily admitted by a writer of eminence!

But with regard to this clock, it is farther added, that Tycho Brahé had made use of it in his astronomical observations. Now Tycho Brahé died in 1600, two years after his *Instauratæ Astronomiæ Mechanica* had appeared, in which he describes all his instruments, especially his clocks, complains of the irregularities of the best of them, without ever making mention of single pendulums to measure the time. But I can produce an apposite instance to show, that a clock of Tycho Brahé had actually undergone the alteration before mentioned. The Ambassador of the Court of Denmark at the Hague was in possession of such a clock at the time of Huygens: it bore the date 1576, and had belonged to Tycho Brahé. Huygens, on first seeing it, wrote in his *Adversaria*, which are preserved (*Leyden MSS.*): "No mention of pendulums (in Tycho's Mechanics). On the clock of Mr Crag, the Danish Ambassador, is the year 1576, if I remember right. But if Tycho had already at that time discovered the application of the pendulum, how comes it that he never, during the twenty-four years that he lived after that period, once mentioned in his writings such a valuable and wished for discovery? I suppose, therefore, that a pendulum was afterwards affixed to Mr Crag's Tychonian clock, designedly to make it seem as if it had been thus formerly constructed." A little below we find, and, as the colour of the ink indicates, written on a subsequent occasion: "That this is really so, the celebrated Roemer, when he came from Denmark to the Hague, has testified to me, and that he knew with certainty when it had been done." He adds, "and by whom," in his manuscript *Anecdota*, where he relates the same again.

In confirmation of his reasoning on the improbability that Tycho being in possession of this capital invention, it should never have become [illegible] I shall farther observe, that the numerous disciples o[illegible]er, and the many learned men of every country [illegible] and examined his apparatus, would infallibly have had some intelligence of it. Among the latter was Blauw, afterwards one of the commissioners appointed by the States to examine Galileo's proposals about the longitude; in their answers, we find remarks on the imperfections of Galileo's proposed pendulum, but not a word of their having ever heard of a better method.

I think it right to inform the class, that among the letters addressed to Huygens, I found one containing a claim to the discovery from a totally different quarter. It is written by Mr Carcavi, a man of rank and merit at Paris, who became one of the first members of the *Academie des Sciences*, at its establishment. He relates having seen an inhabitant of Angouleme, who told him he was in possession of a pendulum clock, made as far back as 1615 or 1616 by a German, for the Queen Marie de Medicis, whose departure, however, had prevented its coming to her; that the artist dying, he had purchased it from his heirs. I mention this as an insulated fact, for as no writer has preserved it, and as subsequent letters of the same gentleman do not allude to it any farther, we have no means of judging whether or not the report deserves any credit at all, and whether what was seen was really a pendulum, by whom it was invented, and how the whole invention came to be buried in oblivion.

Having now gone over the whole evidence, as at first proposed, I believe I shall be warranted in coming to the following conclusions:

That Galileo, after having discovered the properties of the pendulum, and the theory of its vibrations, was likewise the first who showed its use in measuring determinate intervals, and indicating the minute subdivisions of time, an example which was soon followed by all astronomers. That Galileo had thought on a means by which the pendulum might itself indicate the number of oscillations it had performed, without the necessity of counting them, and of course constantly watching it: That he actually conceived the idea of connecting the pendulum with a set of wheels for that purpose, which, however, he only threw out on paper, without putting it in practice, and that he was totally ignorant of the principle of *reciprocal action* between the crown-wheel and pallets, by which the former is regulated, whilst the latter are prevented from returning to rest: That the labours of his son Vincenzio Galilei were such as never to have led to a decisive or satisfactory result, that they were scarcely known, and never came to the knowledge of Huygens: That the attempts of others seem likewise to have failed of complete success, so that even Hevelius, who approached nearest to it, acknowledges in his *Machina Cœlestis*, that, before obtaining it, he was anticipated by another: That it was Huygens who first of all, in December 1656, found out the perfect way of connecting the motion of the pendulum with that of a clock: That he made known the discovery to his numerous correspondents in 1657, published the description, and sent specimens of it everywhere in 1658, and afterwards laboured incessantly to improve upon the adaptation of his principles: That the invention was no sooner known than it was adopted, and balances in a short time removed from all clocks to make room for the pendulum, from which it has happened that many clocks were afterwards found having a pendulum, which nevertheless, by the date, appeared to be of a much earlier construction.

I shall conclude by taking notice of an opinion of the late celebrated Ferdinand Berthoud, expressed in his *Histoire de la Mesure du temps*, p. 101., that Huygens has indeed all the merit we ascribe to him; but that this does not amount to an *invention*, and he refuses him the rank of *inventor*, about a contrivance which he contends is nothing but a mere *substitution* of the pendulum to the balance, the mode of acting by means of pallets being perfectly the same in both. This opinion, which Mr Delambre has already been at some pains to refute, in the *Memoirs of the Institute* for 1808, appears scarcely fair, when it is considered that a great difference exists between balances which acted merely by their inertia, and the pendulum, which is possessed of a principle of motion independent of the clock; and it is farther considered, how admirably this independent motion is combined with that of the clock, regulating instead of disturbing it, and being itself kept in motion by it, without, however, losing any part of its independent rate. Nor was the substitution, as originally made and proposed in the *Horologium*, where the a[illegible]ith the pallets stands upright, so obvious or so easily [illegible] afterwards appeared in the improved form give[illegible]*um Oscillatorium*, where the arbor lies horizontal, and the pendulum takes the place of a perpendicular balance. Mr Berthoud seems to have founded his view of the subject only on a comparison of the latter. The simplicity, too, of the alteration, is rather a proof of ingenuity, especially when we remember that improvements on clocks and pendulums were a favourite theme with the learned at that time.

Notice respecting Plate I. *Fig.* 5, and 6. These figures are intended to represent the rude sketches sent to Huygens from Florence, through the medium of his friend Bouillau at Paris, and of which complete *fac similes* are added to the original memoir. Those here given, without pretending to the same exactness, are tolerably accurate copies, being reduced to nearly $\frac{1}{3\frac{1}{2}}$ or to between *one-third and one-fourth* of the linear dimensions.—*Translator.*

Art. VI.—*New Inquiries into the Laws which are observed in the Distribution of Vegetable Forms.* By Baron ALEXANDER HUMBOLDT [*]. (Continued from Vol. VI. p. 289.)

It is with the distribution of organic beings as with all the other phenomena of the physical world. In the midst of apparent disorder, which seems to arise from the influence of a multitude of local causes, we discover the immutable laws of Nature, as soon as we cast our eyes over a great extent of country, or employ a mass of facts with which the partial disturbances mutually agree. I have had the satisfaction to see this work completed by a minute examination made in Germany, England, Italy, and of late in Denmark. One of the greatest botanists of our times, and indeed of any age, Mr Robert Brown, has compared each of the numerical results with those given by the rich herbaries which he has had the opportunity of consulting. Many of the numbers have been rectified, while the others have been confirmed by an agreement somewhat unexpected. The mass of facts is increased by the very desire of invalidating or corroborating the results to which I have applied myself. It is thus that, in the progress of the physical sciences, general ideas, which at first have only been deduced from a small number of facts, compel observers to multiply the partial results. Enriched by these materials, profiting daily by whatever truth or utility is contained in the most severe criticism of my works, I have been enabled to give to the numerical results of which the table of vegetable forms consists, a degree of exactness which I have not till now been able to attain. It is in the nature of these researches, that we are not able to rectify the *co-efficients* otherwise than progressively, in proportion as the observations accumulate. I have attended here only to the general development of principles. As this kind of botanical arithmetic demands more minute discussions of the proportions of each family of vegetables tc the whole mass of phænogamous plants, I have thrown together these discussions in the notes which I have published separately *.

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[*] This continuation is taken from a Memoir read before the Academy of Sciences on the 19th February 1821.

We may foresee, that the labour which I have bestowed on the families of plants, will one day be applied with success to most of the classes of vertebral animals. The immense collections which are to be seen at Paris, in the Museum of Natural History, show that we are already acquainted with nearly 56,000 species of Plants, cryptogamous and phænogamous; 44,000 *Insects*, 2500 Fishes; 700 Reptiles; 4000 Birds; and 500 species of Mammalia. Agreeably to the inquiries which M. Valenciennes and I have made, there exist in Europe alone nearly 80 Mammalia, 400 Birds, and 30 Reptiles: it follows, that, in this northern temperate zone, there are five times as many species of Birds as of Mammalia, as there are (in Europe) five times as many Compositæ as Amentaceæ and Coniferæ; five times as many Leguminosæ as Orchideæ and Euphorbiaceæ. The beautiful collections lately brought from the Cape of Good Hope by M. De Lalande prove, (if we compare them with the works of MM. Temminik and Le Vaillant), that in that part of the southern temperate zone, the Mammalia are to the Birds in the proportion of 1 : 4,3. Such an agreement between two opposite zones is abundantly striking. The Birds, and especially the Reptiles, increase towards the equatorial zone in a much more considerable degree than the Mammalia. According to the discoveries of M. Cuvier regarding fossil bones, we may be induced to believe, that these proportions have not been the same in all ages, and that, amid the ancient catastrophes of our planet, a much greater number of Mammalia has been lost than of Birds. M. Latreille, in an excellent memoir on the Geographical Distribution of Insects, has not compared the number of articulated animals with the number of plants, and with that of the different classes of vertebral animals which inhabit the same climates; but he has exhibited, in an interesting manner, a view of the increase or diminution of the particular groups of insects, in advancing from the pole towards the equator. I pass over in silence the laborious researches of M. Illiger on the Geography of Birds *. The author has discussed the habitation of upwards of 3800 species; but he has contented himself with viewing them according to their distribution among the five divisions of the world,—a method by no means philosophical, and altogether improper for investigating the influence of climate over the development of organized beings. All the continents, with the exception of Europe, extend from the temperate to the equatorial zone: the laws of Nature cannot, therefore, manifest themselves, when we group the phenomena according to divisions which are arbitrary, and which depend, so to speak, simply upon the difference of meridians. It is not my intention to push to a greater extent these considerations regarding the numerical proportions of animals of different classes. I am satisfied with having called the attention of the learned to a branch of natural philosophy which has appeared to me very worthy of investigation. We conceive how, in a given space of country, the *individuals* belonging to different tribes of plants and of animals may be *numerically limited*,—how, after an obstinate struggle, and long wavering, a state of equilibrium is established, which results from the necessity of food, and the habitudes of life; but the causes which have *limited the forms*, are hidden under the impenetrable veil which conceals from our sight all that extends from the origin of things to the first development of organic life.

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* See *Dictionnaire* *-elles*, arranged by the Professors of the Garden of Plants, vol.

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* *Memoirs of the Academy of Berlin* for the years 1812 and 1813, p. 221,—237.

On examining in detail all that we already know regarding the proportion of monocotyledonous to dicotyledonous plants, we find that the denominator becomes gradually smaller (and this with a very great degree of regularity), in proceeding from the equator toward the 62d degree of N. Lat. It increases probably anew in regions still more northern, on the coast of Greenland, where the Gramineæ have become very rare (*Congo*, p. 4.). The proportion varies from $\frac{1}{5}$ to $\frac{1}{6}$ in the different parts of the tropics. In upwards of 3880 phænogamous plants of Equinoctial America, found by M. Bonpland and myself in flower and in fruit, there were 654 monocotyledones, and 3226 dicotyledones: hence the great division of monocotyledones would be $\frac{1}{6}$ of the phænogamous plants. According to Mr Brown, the proportion is generally in the Old Continent (in India, Equinoctial Africa, and New Holland,) $\frac{1}{5}$.

In the temperate zone, we find (according to my *Proleg.* p. xii. and the partial results published by M. Decandolle, *Dict. des Sciences Nat.* vol. xviii. p. 594,—597.) that the monocotyledones are to the dicotyledones,

In Barbary,	= 1 : 4,8	In France,	= 1 . 4,7
Egypt,	= 1 : 5,0	Germany,	= 1 : 4,0
Caucasus and Crimea,	= 1 : 6,0	Switzerland,	= 1 : 4,3
Kingdom of Naples,	= 1 : 4,7	The British Isles,	= 1 : 3,6
The State of Venice,	= 1 : 4,0	North America,	= 1 : 4,6

In the frigid zone, the proportion is,

In Lapland,	= 1 : 2,8
Iceland,	= 1 : 2,8

We see, that, from the tropics to the pole, the relative increase of the monocotyledones is very regular. As the monocotyledones affect moisture, they are more numerous in the British Isles, and more rare in nd on the dry mountains of Caucasus. I have already o n the Alps of Switzerland, up to the region of th the monocotyledones are to the phænogamous plants as 1 : 7, while in the plains, they are, at the foot of the Alps, as 1 : 4,3, (*Prolegomena*, p. lii.)

In the most fertile part of Europe, at the centre of the temperate zone, an extent of country of 30,000 square leagues produces about 6000 species of plants, of which 2200 are acotyledonous or cryptogamous, and 3800 phænogamous. Among the last there are nearly 500 Compositæ, 300 Gramineæ, (exclusive of the Cyperaceæ and Junceæ), 250 Leguminosæ, and 200 Cruciferæ; but only 70 Amentaceæ, 50 Euphorbiaceæ, and 25 Malvaceæ. The great families constitute from $\frac{1}{7}$ to $\frac{1}{20}$, the small ones about $\frac{1}{80}$ of the total mass of phænogamous plants. This is, so to speak, the mean state of the vegetation in Europe, in the fertile countries, between 42° and 50° of N. Lat. To convince the most incredulous of the reality of *fixed proportions*, or of the regularity to be observed in Europe in the distribution of forms in a particular zone, I shall here present the proportions given by two neighbouring countries, France and Germany. The figures marked in the following Table may be regarded as the coefficients of each family; for, on multiplying the number of phænogamous plants of the temperate zone of Europe by 0.076 or 0.053, the number of species which compose the families of Gramineæ or Cruciferæ may be found.

Compositæ,	Fr.	$\frac{1}{7.4}$	= 0.135	↓
	Ger.	$\frac{1}{8}$	= 0.125	
Glumaceæ,	Fr.	$\frac{1}{7.9}$	= 0.127	↑
	Ger.	$\frac{1}{7.1}$	= 0.141	
Gramineæ alone,	Fr.	$\frac{1}{13}$	= 0.077	↑
	Ger.	$\frac{1}{13}$	= 0.077	
Leguminosæ,	Fr.	$\frac{1}{16}$	= 0.063	↓
	Ger.	$\frac{1}{18}$	= 0.056	
Cruciferæ,	Fr.	$\frac{1}{19}$	= 0.052	↓
	Ger.	$\frac{1}{18}$	= 0.056	
Umbelliferæ,	Fr.	$\frac{1}{21}$	= 0.048	↓
	Ger.	$\frac{1}{22}$	= 0.046	
Labiatæ,	Fr.	$\frac{1}{24}$	= 0.042	↓
	Ger.	$\frac{1}{26}$	= 0.038	
Cyperaceæ alone,	Fr.	$\frac{1}{27}$	= 0.037	↑
	Ger.	$\frac{1}{18}$	= 0.056	
Amentaceæ,	Fr.	$\frac{1}{50}$	= 0.020	↑
	Ger.	$\frac{1}{40}$	= 0.025	
Orchideæ,	Fr.	$\frac{1}{67}$	= 0.015	↓
	Ger.	$\frac{1}{43}$	= 0.023	
Boragines,	Fr.	$\frac{1}{74}$	= 0.014	↑
	Ger.	$\frac{1}{72}$	= 0.014	
Rubiaceæ,	Fr.	$\frac{1}{73}$	= 0.014	↓
	Ger.	$\frac{1}{70}$	= 0.014	
Euphorbiaceæ,	Fr.	$\frac{1}{70}$	= 0.014	↓
	Ger.	$\frac{1}{100}$	= 0.010	
Junceæ,	Fr.	$\frac{1}{83}$	= 0.012	↑
	Ger.	$\frac{1}{91}$	= 0.011	
Ericeæ,	Fr.	$\frac{1}{123}$	= 0.008	↑
	Ger.	$\frac{1}{90}$	= 0.011	
Malvaceæ,	Fr.	$\frac{1}{140}$	= 0.007	↓
	Ger.	$\frac{1}{250}$	= 0.004	
Coniferæ,	Fr.	$\frac{1}{192}$	= 0.005	↑
	Ger.	$\frac{1}{269}$	= 0.004	

This agreement in the greater part of the results is the more striking, that the coefficients have been obtained from very unequal masses of plants. In France 3645, in Germany only 1844 phænogamous plants have been employed, in order to determine the particular proportions of the families. Since the two countries border on each other, the species should in like manner be much the same. The agreement of the results within limits thus marked (the greater part commonly within $\frac{1}{8}$ of difference), prove two facts equally remarkable: 1*st*, That the 1700 to 1800 species of phænogamous plants which the French catalogue has more than the excellent catalogue of M. Schrader employed for Germany, are distributed among the different families almost in the same proportions which we observe among the plants common to the two countries: 2*d*, That the species of Leguminosæ, of Cruciferæ, and of Umbelliferæ, which Germany would appear to possess exclusively, are substituted in France by a nearly equal number of species belonging to the same families. In general, where the greatest discrepancies are observed, they may be attributed to the circumstance, that Germany is situate more to the north than France is. We know that the Cyperaceæ and the Ericeæ increase so rapidly towards the north pole, that, while in the temperate zone the Cyperaceæ are $\frac{1}{20}$, and the Ericeæ $\frac{1}{100}$, in the frigid zone there is $\frac{1}{9}$ of Cyperaceæ, and $\frac{1}{25}$ of Ericeæ. On the other hand, the proportions of the Orchideæ, of the Malvaceæ, and of the Euphorbiaceæ, increase with equal rapidity towards the south. On comparing the preceding table with the table of the three zones, (the torrid, the temperate, and frigid), we observe the same laws. I have added to this comparative table of the vegetation of France and Germany the arrows which, in the general table, indicate the *directions of increase*, from the pole to the equator, and from the equator to the pole. It is a very remarkable circumstance, also, that the coefficients of the families do not change so much, if, instead of examining extensive countries which have from 2600 to 3800 species of phænogamous plants, we confine our researches to an extent of some square leagues; [illegible]mple, to the Flora of Berlin, which, according to the [illegible]nth, comprehends only 900 species. In that [illegible]and, the Leguminosæ are $\frac{1}{19}$, (in the whole of France $\frac{1}{18}$, in the whole of Germany $\frac{1}{18}$); the Glumaceæ $\frac{1}{8}$, (in France $\frac{1}{7.9}$, in Germany $\frac{1}{7}$,) of the whole mass of phænogamous plants.

In the same way that the *system of climates* of the New Continent differs essentially from that of the Old, on account of the unequal distribution of the heat among the different parts of the year, so also the *system of agroupment* of American plants has its peculiar features. It is to the new researches of *botanical arithmetic* that we owe the knowledge of these contrasts between the temperate zones of the two worlds. I have thrown together in the following Table the results of the American Flora of Pursh and of the French Flora of M. Decandolle. I have added certain coefficients of the European frigid region, in order to show how much of a *boreal character* the American temperate region presents in the five families of Ericeæ (and Rhododendra), Coniferæ, Amentaceæ, Umbelliferæ, and Labiatæ.

	Temp. America.	France.	Lapland.		Temp. America.	France.	Lapland.
Compositæ,	$\frac{1}{8}$	$\frac{1}{7}$	0	Malvaceæ,	$\frac{1}{123}$	$\frac{1}{140}$	0
Glumaceæ,	$\frac{1}{8}$	$\frac{1}{7.9}$	0	Labiatæ,	$\frac{1}{40}$	$\frac{1}{24}$	$\frac{1}{70}$
Gramineæ alone,	$\frac{1}{10}$	$\frac{1}{13}$	0	Ericeæ and Rhododendra,	$\frac{1}{36}$	$\frac{1}{123}$	$\frac{1}{25}$
Junceæ alone,	$\frac{1}{123}$	$\frac{1}{83}$	0	Umbelliferæ,	$\frac{1}{37}$	$\frac{1}{20}$	$\frac{1}{25}$
Cyperaceæ alone,	$\frac{1}{40}$	$\frac{1}{27}$	0	Amentaceæ,	$\frac{1}{23}$	$\frac{1}{30}$	$\frac{1}{31}$
Cruciferæ,	$\frac{1}{62}$	$\frac{1}{19}$	0	Coniferæ,	$\frac{1}{103}$	$\frac{1}{200}$	$\frac{1}{180}$
Leguminosæ,	$\frac{1}{19}$	$\frac{1}{18}$	0				

The differences which are manifested in this table between the two continents, bear, not merely upon the five last families, which might be called *boreal forms*, but also upon the Cruciferæ, the Junceæ, and the Cyperaceæ, which are equally rare in the torrid zone and in the temperate zone of the New Continent.

It is conceived that the inquiries regarding the numerical proportions of vegetable families, will present results much more interesting when the Floras of different countries shall be circumscribed within more precise geographical limits, and when botanists shall attend more particularly to the principles according to which varieties and species ought to be distinguished. The catalogues which we see under the vague name of *Flora of the United States of America*, comprehend countries placed in very different climates, from 18° to 9° of mean temperature. Here we have a difference of climate as great as in Europe between Calabria and Austria. When the vegetation of South Carolina, of Pennsylvania, and of New England, shall be described individually, and with the same degree of exactness, we shall notice a regular increase and decrease in the numerical proportions of families from south to north. At present, we only know the *general mean* of these partial proportions. Many countries seem richer in plants, because the botanists have inconsiderately elevated varieties to the rank of species. In another country, the travellers often neglect the plants which they believe to be the same as those of their own country. But when we attend to the great divisions, and when the number of species which we compare is very considerable, our researches are favoured with happy compensations. It is thus that the new Floras, much more complete of America and Lapland, published by Messrs Pursh and Wahlenberg, have not sensibly altered the numerical proportions which we find on confining ourselves to the old Floras of Michaux and Linnæus, (*Berl. Jarhb. der Pew.* b. i. s. 24.). Whatever may be the corrections which shall be made in my work, I am persuaded beforehand, that the more that exact observations are accumulated, and the more we look beyond the same hemisphere, the same *system of agroupment*, the partial variations of the coefficients will be found not to be made by sudden starts, but according to invariable laws. It may be that the *tropical proportion* of the Malvaceæ is $\frac{1}{32}$ or $\frac{1}{33}$, instead of $\frac{1}{35}$, but it is not the less certain, that the Leguminosæ and the Malvaceæ increase toward the equator, while the Junceæ and the Ericeæ increase toward the pole. One may recall in doubt the *quantities* of the variations, the *rapidity* of the increase, but not its *direction*.

On comparing the coefficients which belong to the same families, in different zones, we find, in the rapidity of the increase, contrasts strongly marked. In the Old Continent, the proportions of the Gramineæ, the Leguminosæ, and the Euphorbiaceæ, change much less from the temperate zone to the equator, than from the temperate zone to the pole.

Those who are accustomed to consider each phenomenon in an absolute and [redacted] oint of view,—who regard the mean temperature of which are observed in the variations of terrestri the proportions between births and deaths, as daring hypotheses, and as vague, theoretical speculations,—will probably look with contempt on the discussions which form the principal object of this memoir. Those, on the other hand, who delight to contemplate the mutual concatenation of organized beings,—who are aware that the numerical results correct themselves by the accumulation and the careful investigation of particular facts,—will cherish a kind of research, which throws light upon the economy of Nature,—on the connection which is observed between the climates and the form of beings,—on the distribution of plants and of animals in the different regions of our planet. It is only by the numerical examination and the comparison of species, that we can form a correct idea of the state of the vegetation of a given country;—of the general influence which the temperature exercises over the frequency of certain forms, near the equator, in the mean parallel, and towards the polar circle;—of the characteristic features which distinguish, in isothermal zones, the two systems of agroupment of the Old and the New World *.

ART. VII.—*Notice respecting Roman? Vessels found in Scotland on the Estates of Sir* JOHN HAY, *Bart.*

THE two figures Plate II. Fig. 3. and 4. represent vessels found on the estates of Sir John Hay, at Linton and at Eshiels in Tweeddale.

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* For the use of such as have not made descriptive botany their particular study, and who are yet desirous of acquiring a knowledge of the investigations which have been attempted in the different branches of the natural sciences, we shall add in this place the names of some of the most common plants which characterise, if we may so speak, the tribes or families which are frequently the subject of discussion in this memoir. (The translation of the following is modified so as to give an English reader an idea of the families). *Junceæ* (Rushes); *Cyperaceæ* (Hard or Moor Grasses, Cotton-Grass); *Gramineæ* (Corn, Grasses); *Compositæ* (Dandelion, Thistles, Sunflower); *Leguminosæ* or *Papilionaceæ* (Vetches, Pease, Clover); *Rubiaceæ* (Rennet, Madder); *Euphorbiaceæ* (Sun-spurge, Dogs' Mercury); *Labiatæ* (Mint, Thyme, Rosemary); *Malvaceæ* (Mallows, Hollyhock); *Umbelliferæ* (Carrot, Hemlock, Chervil, Carraway); *Cruciferæ* (Mustard, Cresses, Radish, Turnip).

The great mass of plants which cover the globe is divided by botanists into *Phænogamous* (those having visible flowers), and *Cryptogamous* or *Agamous* (Ferns, Lichens, Mushrooms).

Both of these were imbedded in the peat; and the smallest, which resembles a modern coffee-pot, is from the last of these places. A leg had been broken from it, which was replaced with a new one by the person into whose possession it first came. It has been cast in one piece, and shews that the workmen were expert in the art of moulding. That it was intended for boiling some fluid which was to be used hot, is plain enough.

The other is a common cooking-pot, the very model of our own. It is a much ruder piece of casting than the last, but is extremely substantial and weighty, as indeed is the former. It is 10 inches high: the coffee-pot is 8.

Both of these vessels are made of the same composition of tin and copper as the celts, and other well known instruments of the Romans, commonly called Roman Bronze.

It is probable that they are Roman utensils, as these places also lie within the limits which this people occupied when in Scotland. The remains of a station have indeed been traced near to Linton.

EDINBURGH, *April* 1822.

ART. VIII.—*Some Account of those Manuscripts in Great Britain, which contain the Greek Text of the Mathematical Collections of Pappus.* By S. P. RIGAUD, Esq. M.A. F.R.S. Savilian Professor of Geometry, and Professor of Experimental Philosophy in the University of Oxford, &c. &c. Communicated by the Author.

THE Mathematical Collections originally consisted of eight books. Fabricius, indeed, in his Bibliotheca Græca, seems to think that the number might have been greater, but he speaks with hesitation, "nam in præfat. ubi duodecimum librum allegare videtur (pag. 252.), incertum est propter luxata versionis verba, an alterius potius scriptoris (Euclidis fortasse) librum duodecimum elementorum intelligat." The passage referred to occurs in the end of the preface to the seventh book, and if Fabricius had lo [illegible] Greek text, as given by Halley in in his edition of [illegible] sectione rationis, he probably would have ente[illegible] t. Some of the manuscripts read in this place εν δωδεκάτω τωνδε των ϛοιχειων, and the text was most probably the same in the MS. used by Commandine, "solertissimo mathematico," as Meibomius remarks, "Græcæ linguæ mediocriter perito;" but Halley rejects τωνδε altogether, and, if we admit it to be an interpolation, there will remain no reason for supposing that Pappus meant to refer to his own work. Τα ϛοιχεια is the well known expression for the Elements of Euclid; Pappus himself talks of Ευκλειδȣ τȣ ϛοιχειωτȣ; the title uniformly given to each of his own books is συναγωγη, and at the end of Jos. Scaliger's MS., which is now at Leyden, the whole is closed by των συναγωγων Παππȣ τελος.

Of these eight books, it is very probable that no entire copy is at present in existence: certainly there is none in our own country. In the Catalogus Librorum MSS., Angliæ et Hiberniæ, published at Oxford in 1698, there is mention made of two manuscripts in the Savilian Library in that University. They are marked 6550 : 3, and 6556 : 9, the larger numbers referring to the general enumeration, and the smaller (3. and 9.) to that of the Savilian Manuscripts. These have been about 200 years in Oxford; for they appear in the original "Catalogue indented betweene the Universitye and Sir Henrie Savile, contayning the names of such bookes as the said Sir Henrye Savile hath bequeathed to the University, for the use chiefly of the mathematical readers." This catalogue is signed by Sir H. Savile himself, and there can therefore be no doubt of the manuscripts being older at least than 1620. This is indeed a late date, but neither of the books bears any marks of high antiquity.

No. 3. is very well written on paper, and the ink is clear throughout; the volume is in excellent preservation, and in the original binding. It contains the Greek text of the 3d, 4th, 5th, 6th, 7th and 8th Books of the Mathematical Collections, with the diagrams for these books, very neatly drawn, after which are inserted some works of Theodosius and Autolycus. The whole is written undoubtedly by the same hand: each line is regular, each page is bounded by an ample margin, and the extremities of the writing are as even as if they had been set off with a ruler. The titles of the several books, those of the leading divisions of the work, many of the initial letters, and the numbers of the propositions in the margin, are written with red ink.

The book is in folio, the paper is all of the same texture, although the water-marks are not all the same: no date, however, appears, and there is no notice, in any part of the volume, of when or by whom it was written; there is no memorandum in it of any one, to whom it has ever belonged; nor are there any traces of where it was procured by Sir H. Savile. There is in the same library a volume containing some works of Ptolemy, Theon and Aristides Quintilianus, which is evidently by the same transcriber; and his hand again appears in a copy of Simplicius's Commentary of the second of Aristotle's books *περι ουρανου*, but no further light can be collected from any of them. In No. 3., many of the passages, which were originally deficient, have been filled up in pencil; there are likewise many memoranda written in the margin in pencil; there are some also in ink in a smaller hand, which are most numerous in the beginning of the 4th book.

No. 9. is not by any means so well written as No. 3. The mathematical collections occupy the latter part of this volume: they are entirely written on separate half sheets of folio paper, which have afterwards been pasted on guards, in order that they might be bound up with the other manuscripts. The book is described in the Catalogus Librorum MSS. above mentioned, as "Pappi Alexandrini Coll. Math. deest. liber 1. et septimus et initium 2." This is copied by Harles in his edition of Bibliotheca Græca, and he probably had not the means, even if he had suspected the inaccuracy, of correcting it; but it is extraordinary that a great mistake should have been made in this description. The catalogue of the Savilian Library was drawn up for this collection by Caswell, who published a short treatise on trigonometry at the end of Wallis's Algebra, and who became Savilian Professor of Astronomy in 1709; but he did his work most incorrectly. In the present instance the error is remarkable. If he had overlooked any deficiency, it might easily have been accounted for; but that he should have gone out of his way to mark a deficiency, which never existed, is very extraordinary. The manuscript does not want the seventh book; neither has that book been inserted at any time subsequent to the formation of the catalogue. The writing of it agrees exactly with that of the rest of the work; and Wallis in 1688 describes this MSS. as one "qui continet non tantum Pappi librum tertium *cum sequentibus*, sed secundum etiam, non quidem integrum, sed ipsius partem non contemnendum." Halley likewise specifically says, in the preface to his edition of the Conics, "singulis Apollonii libris Pappi Lemmata præfixa dedimus, e *duobus* codd. MSS. Savilianis desumpta;" and these lemmata are all in the seventh book, comprising Prop. 165–234. of Commandine's translation. Halley also prefixed to his edition of Apollonius de sectione rationis, the Greek text of the preface to the seventh book, "pristinæ integritati, quoad ejus fieri potuit, restitutum e *duobus* codd. MSS. Bibliothecæ Savilianæ." Now, these two works of Halley were published in 1710 and 1706, and the date of the catalogue is 1698; if, therefore, we had no other ground of argument, we could hardly imagine that he would have derived his authority without any notice, from so recent a manuscript as that of the seventh book must have been, if it had been inserted subsequent to Caswell's publication. The several books, however, in No. 9. are not arranged in their natural order; which, though not sufficient to account for the assertion of the defect, may have prevented the blunder from being immediately detected. In the beginning we have "Pappi Alexandrini συναγωγη . . . λειπει το πρωτον και η αρχη του δευτερου;" the first words of the text are γαρ αυτους ελασσονας μεν ειναι εκατονταδας, which is the middle of the enunciation of the 15th proposition; and then follows the end of the second book, as published by Dr Wallis. The other six books then come in the following order, 3, 7, 5, 6, 8, 4. The beginning of the 8th book is written on the back of the leaf, which contains the end of the 6th; it is not therefore improbable that the MS. was originally copied from one which wanted the 7th and possibly the 4th book, and that these were afterwards supplied and misplaced; but the whole is clearly of the same age, and written by the same persons. After a careful examination, there seem really not to be more than four or five; but their several hands are so mixed together, often two, and sometimes more, occurring in the same page, that there appears, at first sight, to be a larger number. The best of these writers executed the greater part; of his coadjutors one writes αναλλογιαν παρραλληλον, and, although these blunders are generally corrected, and are, for the most part, confined to the beginning of his work, still they shew the man not to have been familiar with Greek. Another, who certainly writes a more practised hand, fills his parts with contractions: these are not only literal but verbal; we have ⊙ for ηλιος, △ for τριγωνον, &c. &c.; again, φανερον is φ^{ον}, ελαχιϛος is ελα^{ϛος}, and αρ^{κης} occurs where we can only determine from the context whether it is intended for αριθμητικης or αρμονικης. What the arrangement was which these scribes made among themselves, is not easy to conjecture; but it seems to have been such, that they might all, at least in some parts, have been working at the same time; for the pages are hardly any of them full, some having more and some less vacant spaces left at the bottom of them, and the last lines of the pages are very seldom, if ever, complete, although the change to the next page may be in the middle of a sentence, or even of a word. All this may have been occasioned by the copy's being made to contain exactly what was found on each page of that from which it was taken; and as the diagrams are in general omitted, this alone, if we suppose them to have been annexed to the several propositions in the original, will account for inequalities in the length of the pages in the transcript. The plan of several copying different parts at the same time, may account for a very singular confusion which occurs in the 4th, 5th, and 8th books, in which some of the pages are divided into two, while the upper and lower halves do not make parts of the same passages, neither does the text in the successive pages follow any regular order.

The volume is put together in a parchment cover, without any boards, and is, upon the whole, in as good condition as could be expected; but the lines generally reach both ways to the edges of the paper, so that letters are sometimes lost on one side by the pasting of the leaf on the guard, and, on the other, by the outer margin's being worn. There is no memorandum of the age of the manuscript, nor of the persons who were employed to copy it. In Sir H. Savile's own catalogue, the Pappus is mentioned by itself, so that the other books were probably bound up with it at a subsequent period. There is the following memorandum written against the Pappus: "m. script. "Argentorati," and as Dr Trail speaks of a MS. in the Strasburg Library which is not noticed by Harles, it is possible that there may be some connection between the books. In the 24th, and some other pages, there are references to Commandine, which appear to be written with the same ink as the manuscript; the difference of the Greek character from the Latin, in which these references are written, making it impossible to form any opinion from the handwriting; but whether they are or are not by the original transcribers, the manuscript is probably not older than 1588, when the Latin was first published.

There are no red letters or ornaments of any kind; the lines are uneven, the margins are rugged, the ink is faded in many parts, and in others remains much blacker in some words than in the rest. There are frequent erasures, from the words having in haste been written wrong, and the pen's having been dashed through it, which is not the case in No. 3. The text, likewise, has been corrected in many other places; in some instances this seems to have been done by the transcriber, who has been mentioned as more accurate than his associates; but many alterations have been made by later hands, particularly in the beginning of the 3d book, where the old writing is in some instances absolutely effaced by the corrections, and at p. 15. we find "hactenus recensuit Wallisius."

Halley says, "Græca Pappi in hisce codicibus sæpiuscule luxata sunt et depravata." This is not, indeed, peculiar to the Oxford MSS.; Dr Trail says, that all "which have been examined are mutilated, and contain many errors, from the ignorance or carelessness of transcribers;" and he quotes from Dr Simson, "non pauca ...... in eo codice (Parisiensi Regio sc.) vitiata sunt, ut in omnibus fere Pappi propositionibus, et, ut videtur, in omnibus manuscriptis." Wallis says more specifically of the Savilian Manuscripts, "qui elegantius scribitur, est mendosior; quique festinantius et minus eleganter scribitur est emendatior; ex quo alterum fuisse descriptum conjicio." Now if No. 9. is in some respects more accurate than No. 3., the cause must be found in its having been taken from a more correct original, for it certainly is not from the merits of the transcribers; and the conjecture of the one's being only a fair copy of the other is manifestly erroneous. There are indeed many remarkable points of coincidence; the same errors, the same repetitions of words and sentences, and the same lacunæ occur in innumerable instances; but after a complete and careful collation, I can venture to express a decided conviction of their being independent of each other. This conclusion is not drawn merely from the number of various readings, although it is so very great, as to be nearly irreconcileable with Wallis's conjecture; but there are other and stronger arguments. Though the lacunæ are in general the same, there are some in No. 3. which are filled up in No. 9., and *vice versa*, there are some in No. 9. which are filled up in No. 3.; there are readings in No. 9. which are inserted in the margin of No. 3., with ισως annexed to them, which could not be if No. 3. was only written out on the authority of No. 9. There are only a few of the diagrams in No. 9. and it is by no means clear that these have not been inserted by some one subsequent to the original transcription; whereas the diagrams are complete in No. 3., and it is most probable that the text and the figures were written and made at the same time. Again, the lines are very uneven and irregular in No. 9., and in collating, I have sometimes passed over one of them, and not fixed my eye on that which immediately followed what I had been examining; from the state of the writing this was unavoidable, and yet in no one instance was an omission from such a cause detected in No. 3., whilst, on the contrary, my oversight was always corrected by the occasion of its being found from the text of that manuscript. Many contractions, likewise, and words of which the writing is nearly unintelligible in No. 9., are only to be made out by reference to the analogous passages in No. 3.

Wallis says, " Ex tribus quos Oxoniæ habemus Pappi Alexandrini Codd. MSS. (in Bibliotheca Bodleiana uno et duobus in Saviliana), &c.;" but after repeated and diligent search, no other traces can be found of any such manuscript in the Bodleian library. None is mentioned in any of the catalogues, none appears to have been known to Dr Edward Bernard, who was from 1673 to 1691 Savilian Professor of Astronomy, and was equalled by very few men of his time, in an extensive knowledge of the Oxford manuscripts. There is in the catalogue of English and Irish MSS. a list of some of Bernard's own, which were purchased for the [illegible]n, among which is " Pappi Florilegium Mathematicu[illegible]s quotes this as a Greek MS.; but the list, in whic[illegible] entitled " Libri Græci, cum scriptis codicibus collati aut a viris doctis illustrati;" and there can be but little doubt of the reference's belonging to Bernard's copy of Commandine, which is now in the Bodleian, with a number of marginal remarks and extracts from the Greek text. Bernard, likewise, in his "Veterum Mathematicorum Synopsis," cites only the two Savilian manuscripts of Pappus, and one belonging to D. Lescuyer, who was a French gentleman mentioned by Mersennus; Halley likewise, who was also contemporary with Wallis, makes no mention of any but the two in the Savile Library. But all this, however strong, is only negative evidence, and Wallis's expressions are so very precise, that they can hardly be reconciled with the probability of any mistake. There may still be hopes that the book has been bound up with others, so as to have escaped notice. This is known to have happened in many instances, and if it has done so in the present case, the researches now going on in the Bodleian, for the formation of new catalogues, are the most likely means of bringing this treasure to light.

There is in the Savilian Library the Greek of the third, and of the latter end of the second book, copied from No. 2368 of the French King's Library. These are written very distinctly in a large foreign hand, upon 30 leaves of folio paper, about the size of foolscap, and the diagrams, drawn on a whiter and finer paper, are wafered on to the places to which they belong. What the history of this transcription may be, and why it was carried no farther, we have no clue for discovering. It is not inserted in any catalogue, and is found under the same cover of No. 9., with the transcript of the fragment of the 2d book, which Dr Wallis had prepared for the press. This seems suspicious; after finishing his work, it will occur that he might by mistake have put all the MSS. together into the Savile Library, but still it is clear that this is not the Bodleian MS. to which he referred. There is no water-mark, from which the age of the paper can be collected; but the colour of the ink, and the appearance of the whole, is too fresh for its having been as old as the time of Wallis; besides (which is more conclusive), he says that of the three manuscripts, "fragmentum libri secundi non nisi in unico habetur," and that one is undoubtedly the Savilian No. 9. It is clear, therefore, that the Bodleian MS. did not contain any part prior to the 3d book, and that it consequently cannot have been that which is now under consideration.

(To be concluded in next Number.)

ART. IX.—*Remarks on the Columnar Structure of Trap-Rocks* *. By C. DAUBENY, M. D. M. G. S. Communicated by the Author.

THE late M. Faujas St Fond, in his work on the Vivarais, has given us an interesting account of several of the Coulies of Lava, which, at very early periods, although subsequently to the last retiring of the waters, streamed down from the then existing volcanoes into the valleys situated at their bases.

The perfect manner in which many of these torrents of lava may still be traced from the summit of the mountains, and even from the very craters whence they were ejected, did not excite in my mind a stronger interest than was caused by observing the close analogy which these igneous products often present to the basaltic rocks of countries, in which the existence of volcanoes has never hitherto been made out.

In following the track of these streams from their apparent sources, we are struck with the increasing solidity which they assume in the course of their descent, so that as we approach the valley which constitutes the limit of their progression, they lose by degrees the porous texture that originally belonged to them, and put on the character of a compact, and often columnar form of basalt.

The inferences, with respect to the igneous origin of the latter class of rocks, from the analogous appearances presented by these volcanic products, are of the same description, and *subject to the same limitations* with those I have attempted to deduce in a paper published in the Edinburgh Journal, from my observations in Auvergne; but on the columnar form which this lava so often exhibits, where it occupies the lowest part of the valley which bounds its progress, I intend to trouble the Society with a few remarks.

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* The substance of [illegible] the Geological Society, June 2. 1820.

It is on the above fact, indeed, that M. Faujas has founded an opinion as to the cause of the prismatic arrangement which basaltic rocks exhibit; for, as in the narrow and steep defiles of the Vivarais, the bottom of the valleys is usually occupied by a running stream, he conceives that the columnar structure developed in the lava in these situations, arises from the sudden congelation produced among the particles of the liquified mass, by falling into water, and seems disposed to extend the same hypothesis to basaltic rocks in general. This theory, however, appears not only quite untenable as applied to basaltic rocks in general, but even very questionable when confined to the case we are at present considering. Sudden congelation, so far from being favourable, as M. Faujas supposes, to a regular arrangement among the particles of matter, proves, so far as we know, almost destructive of it.

Without entering, however, into the merits of this theory, which I conceive will find but few supporters, it will be sufficient for my present purpose to remark, that the columnar structure of the lava found in the bottom of the valleys in the Vivarais, must have been exposed by some process subsequent to its original formation, for the bed of the existing rivulet, which exhibits so many beautiful examples of columnar basalt, appears in every case that I have seen, to have been scooped out subsequently to the flowing of the lava*; for the latter must have blocked up the course of the original stream, if any such existed at that period, as appears from the fact of the lava extending to a considerable height on the opposite side of the valley from that whence it was derived.

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* The abrupt face which the beds of the rivulets present on either side, compared with the more gradual slope of the valley above, seems to afford a beautiful illustration of the difference between the effect produced by streams now existing, and by the retiring of a body of waters considerable enough to cover the whole face of the country. On this account, the Vivarais should be visited by those who feel interested in the theory that has been so much insisted upon, with respect to the origin of a great proportion of our present valleys from the Mosaic Deluge, or at least from the action of a body of waters equally considerable. Possibly, too, an accurate examination of the depth of lava cut through in these instances by the existing streams, might lead to some inferences as to the age at which the lavas must themselves have flowed.

Instead, therefore, of conceiving that water caused a regular arrangement * among the particles of the lava, in consequence of the suddenness with which it cooled them, does not the present case lead us to the supposition, that the *natural* structure of the *stone* has been in this, and probably in other instances, developed by the continued action of the stream, which, in the course of ages, has cut itself a passage through the resisting materials of the rock.

Do we not indeed generally observe the most beautiful examples of columnar trap in rocks, which are either at present exposed to the action of water, or which may, without violence, be supposed to have been formerly in that predicament?

It is somewhat rare, I believe, to meet with any well marked instance of columnar trap in the interior of a mine †; and if the occurrence of these pillars high up on the sides of inland valleys be brought forward as an exception, it should be recollected, that these very valleys have probably been themselves formed by the agency of water.

At the same time, I am far from contending that the structure which we observe in basalt is uniformly attributable to the above cause; or, in other words, that the arrangement natural to the rock has in all cases been so concealed as to require to be developed by mechanical causes *.

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* I have carefully avoided the use of the word "crystal," with reference to the structure existing in trap-rocks, because I am aware that the term might be objected to, as being, strictly speaking, inapplicable to these columnar concretions, in which there is neither a definite number of planes, nor planes meeting at determinate angles. At the same time, when we consider the many points of analogy, it may not be too rash to assume, that the formation of crystals in *simple*, and of distinct concretions in *compound* minerals, both probably depend upon the same ultimate law of matter. Thus, Count Bournon, in his "Traité de Chaux carbonateé," p. 162., considers the fibrous and lamellated structure of that mineral a kind of irregular crystallisation.

† Professor Buckland, to whom I shewed this paragraph, referred me to M. Faujas St Fond's description of the Quarries at Nidermennig, near Bonn, for an instance of columnar lava at a considerable depth below the earth's surface. I have since been at the spot myself, and found the columns extremely irregular, as indeed may be seen in M [redacted] 's plate, in the *Annales du Mus.* vol. i. Their appearance, indeed, is s hat of the basalt of the Vivarais or the Giant's Causeway, that y to bear upon the question, so far as it respects the latter.

As in the case of a simple mineral, the crystalline structure probably always exists, although more commonly it may be observed by the subsequent deposition of other matter; so may we suppose that the materials of which basalt is composed always affect a peculiar structure, notwithstanding the amorphous character which it so often presents; and, on the other hand, it would be as absurd to suppose that this structure was uniformly concealed, until made apparent by the denuding action of water, as to contend that there was no such thing as a crystal of quartz or felspar, until the angles of the originally shapeless mass were exposed by the operation of some extraneous cause.

Yet that, in many instances in which the *columnar* structure *was* concealed in the manner above described, it may since have been developed by the agency of water, appears to me probable *a priori*, from considering that the amorphous portions of a rock are generally of an hardness inferior to those in which a regular arrangement is visible, and that it is therefore natural that the former should be first affected by mechanical abrasion; in confirmation of which idea, I may mention the following facts, observed by myself during my excursion in Auvergne.

The rock, at the foot of which the town of Muret, department of Contal, is situated, exhibits, on the side which overlooks the houses, some remarkably regular basaltic columns, which, towards the summit, are vertical, but, as they descend, appear to be bent in conformity to the slope of the hill. The curve which they describe still increasing, they become by degrees quite horizontal, so that the *terminal planes* only of the prisms jut out from the side of the rock. Now, it is curious, that, in the latter instance, the interstices between the columns are generally filled up by a species of trap much looser in its texture than that of which the columns are themselves composed, the want of consistency in the stone appearing to increase in proportion as it recedes from the prismatic portion of the rock.

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* Some curious experiments have been published by Mr Daniel in the Journal of the Royal Institution, on the manner in which the crystalline structure of minerals may be developed even in amorphous masses by the action of acids, water, and other substances, which possess a chemical affinity for them.

Such experiments seem to afford considerable confirmation to the views I have here offered, in proving that a crystalline structure exists in more mineral substances, although frequently concealed by an upfilling of other matter, and that when this is the case, the latter, being of a looser texture, is *first* acted upon, thus causing the crystalline arrangement underneath to be gradually developed. It will not then be considered improbable, that this process of dissection (if I may so express myself), which, in the cases mentioned by Mr Daniel, appears to have been carried on in our laboratories, should sometimes take place on a grander scale in nature.

In this case, therefore, we seem to have an instance of a rock, the distinct concretions of which are obscured by an upfilling (if I may be allowed the expression) of matter possessing inferior hardness and looser consistency *; that in such cases the abrading action of water would be competent to effect the removal of these softer portions, and the consequent exposure of the structure underneath it, will, I think, appear from the following circumstance, observed in the course of my travels through the same part of France, which furnished me with the fact I have just been mentioning.

At the village of Prentigarde, about half a league from "Les Bains de Mont Dor," a well known watering-place in the department of the Puy de Dôme, the trachytic or porphyry formation, of which I have given a detailed account in my Memoir on Auvergne, is seen surmounted by a basalt of a very compact character. A good section of the two rocks is exhibited at what is called the Cascade du Quereuil, where a small mountain torrent is precipitated down a perpendicular escarpment, to the depth of perhaps 50 feet.

Now, it is curious, that although the basalt on either side is wholly amorphous, being divided only by irregular fissures into imperfect quadrangular masses; yet that, where it lies in immediate contact with the falling rivulet, it exhibits some well-washed columns.

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* I did not observe any of this friable trap filling up the angles between those columns which approached to a vertical position. Under these circumstances, they might have been freely exposed to the denuding action of water at some former period. The columnar structure is therefore here fully developed; but where the columns have, by some singular convulsion of nature, or the operation of some unknown cause, been ren[illegible]orizontal, the amorphous portions filling up the interstices between the co[illegible]me measure protected from the action of the elements, and ther[illegible]main.

It seems clear, then, that in this instance at least, the continued action of water had developed the natural structure of the rock; for, if these columnar concretions existed there originally, why are they limited to that portion of the rock which has been washed by the torrent; or if they depend merely on a peculiar condition of its component parts, how comes it, that the basalt should have all at once undergone such an alteration, as caused it to lose its regularity of form, and that at the very *point* at which it ceased to be affected by the action of water?

There seems, therefore, little ground for doubting that the columnar form of the basalt has, in the present instance, been developed in the manner above mentioned; and the same explanation will, I conceive, apply to the lava of the Vivarais described in a former part of this paper, and perhaps to certain basalts exhibiting the same structure. To what extent, however, this hypothesis is to be received, or how far it may be regarded as accounting in general for the manner in which the internal structure of these rocks has been developed, is a question which I freely leave open for others to decide upon, satisfied with having shewn that the views in which I have indulged, are, to a certain length at least, not wholly hypothetical.

MAGD. COLL. OXFORD,
April 1822.

ART. X.—*Analysis of Tutenag, or the White Copper of China.* By ANDREW FYFE, M. D. Lecturer on Chemistry, Edinburgh.

VERY different statements have been given of the composition and origin of Tutenag, used by the Chinese in the formation of many of their metallic utensils.

According to Keir, it is a white alloy of copper, zinc, and iron, which is very hard and tough, but at the same time malleable, and taking on a fine polish. An inferior sort of it, according to the same author, is more of the colour of brass.

De Guigne, on the contrary, states, that its properties, especially that of imparting to copper a white colour, and rendering it less liable to acquire verdigris on its surface, prove that it does not contain zinc. According to him, it is an alloy of iron, lead and bismuth.

Engestroem, in the Stockholm Memoirs, states, that the *Pak-Fong*, or White Copper of China, is composed of copper, nickel or zinc, the last of which amounts to seven-sixteenths of the whole, and the proportions of the two first are to each other as 5 to 7.

Dr Howison of Lanarkshire was so fortunate, when in China, as to procure a basin and ewer of Chinese or white copper, a part of which he sent me for analysis. From the experiments I have performed on it, I find the composition to be different from what is stated by the above named chemists, its component parts being Copper, Zinc, Nickel, and Iron; the last of which, however, is but in small quantity.

The basin in the possession of Dr Howison is of a whitish colour, approaching to that of silver, and is very sonorous. When held in one hand, and struck with the fingers of the other, the sound is distinctly heard at the distance of an English mile. It is also highly polished, and does not seem to be easily tarnished. The piece that was sent me I found was malleable at a natural temperature, and at a red heat; but when heated to whiteness, it was quite brittle, breaking with the slightest blow of a hammer. By great caution, it was rolled into thin plates, and was drawn into wire, of about the thickness of a fine needle. When fused in contact with the atmospheric air, it oxidated, and burned with a whitish flame, in the same way as zinc does. Its specific gravity at 50° was 8,432.

Five grains of it were subjected to analysis, with the view of ascertaining the proportion of its ingredients; the result was,

Copper, - -	2.02	Or in the 100 Parts,	40.4
Zinc, - -	1.27		25.4
Nickel, - -	1.58		31.6
Iron, - -	0.13		2.6
	00		100.00

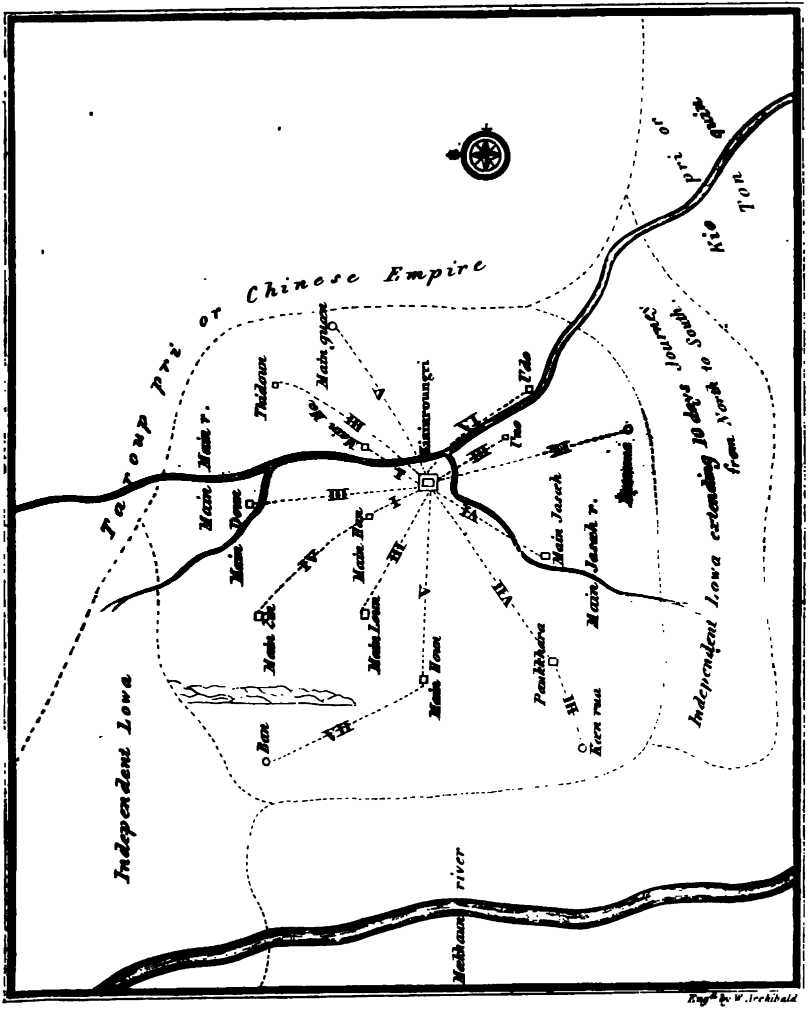
The method wl in preparing white copper, is not known in thi h it seems to be the general

PLATE III.

MAP OF THE TAROUP SHAN COUNTRY.

BY A SLAVE OF THE HEIR APPARENT OF AVA

Edinr. Phil. Jour. Vol. VII. P. 71.



Engd. by W. Archibald

Published by A. Constable & Co. Edinr. 1822.

opinion that it is procured by the reduction of an ore, containing the ingredients of which it is composed. In a letter I received from Dr Howison, he mentions, that Dr Dinwiddie, who accompanied Lord Macartney to China, shewed him, when at Calcutta, several specimens of the ore from which he was told the white copper was procured, and which he obtained at Pekin. The basin in the possession of Dr Howison cost in China about one-fourth of its weight in silver; and the exportation of utensils of this alloy is prohibited. These circumstances also render probable the opinion, that the white copper is obtained by the reduction of a metallic ore, for in China labour is cheap, and the metals composing it are said to be found in great abundance.

ART. XI.—*Account of a Map of the Tarout Shan Territory.* By FRANCIS HAMILTON, M. D. F. R. S. Lond. & Edin. and F. A. S. L. & E. Communicated by the Author.

THIS Map (See Plate III.) I procured at Ava from the same slave who gave me the General Map published in No. IV. of this Journal. In the account given of that map (p. 269.), I have mentioned some circumstances respecting this territory; and, in particular, that this country, by the Mranmas called Tarout Shan, or Siam of the Chinese, is the Chiva of the *Modern Universal History*, (vol. vii. p. 452). Since I wrote that account, I have observed, that this country is mentioned in the *Edinburgh Gazetteer* by the name Laktho. It is true, that Laktho is there said to be situate between Tunquin Laos and the North of China, an error so palpable that it is surprising how it could escape the compiler; for if situate between Tunquin and Laos, it must have the south of China as its northern boundary, as the slave represents in this map.

No hint is given in the *Edinburgh Gazetteer* from what sources the compiler derived his information concerning Laktho; but the state of the country, when the authors which he follows wrote, (probably some of the early Portugese), would appear to have been much worse than was represented to me at Ava; for in the Gazetteer it is stated, that " the population is dispersed in small and rude communities governed by hereditary chiefs, who carry on an almost perpetual war with each other;" but when I was at Ava (1795), the government was said to be vested in one chief, who, although tributary to the Mranma sovereign, and honoured by him with only the title of Zabua, seemed to enjoy considerable power, as his attendance at the Court of Ava to do homage was not insisted on, as was done with the Zabuas of Upper and Lower Laos; and it was acknowledged, that, like the Princes of Ava, sons of the monarch, he was entitled to have the gilded umbrella. From the accompanying map also it would appear, that including the capital, his dominions contained twelve cities (Main), each the seat of a governor, and its territory is stated to extend from three to twelve days' journey in different directions from the capital. Its smallest extent from thence was towards the north and east, between N. W. and S. E., in which directions the most remote places are not more than five days journey from the capital. Towards the south and west, some places are ten, eleven and twelve journeys distant from the same.

It is evident, that the places in the map are not laid down by any scale suited to their respective distances; for Banmue, eleven days' journey from the capital, stands no farther from thence than Main Zin, a town distant only five days' journey. On the whole, it is evident, that the capital should have been placed farther towards the N. E. making the territory extend less than it apparently does towards China and Tunquin, and farther towards Upper and Lower Laos. In fact I consider, that by far the greater part of this territory occupies the space on Mr Arrowsmith's map of Asia on the banks of the Lesien Kian, a great part of which is there represented as within the boundary of China, but as desert and without towns. There is in this part, even within the boundary of China, besides a large part in Tunquin, an empty space east from the Mækhaun River of about 150 German miles from N. W. to S. E., and of about 100 miles from N. E. to S. W., and in this space I consider that the greater part of the Tarout Shan territory is placed, which will account f [illegible] rm Chinese (Tarout) being applied to it, as a large p [illegible] the boundary which the Chinese choose to affix [illegible] This entirely agrees with the present map, which represents the Chinese empire (Taroup Pri) as extending all along its northern and eastern boundary; while Kio Pri or Tunquin comes in to the S. E.

In the Gazetteer, Laktho is said to contain no considerable or navigable river; and, although I have placed it along the banks of the Lesien Kian, which has a course of at least 300 miles in length, yet in all probability it is not navigable, as it is only denominated a Kian, the same word no doubt with the Khiaun of the Mranmas, which implies a small river, that is not navigable, unless it be during the periodical rains, when every ditch is swollen, so as to convey boats or canoes. In the map, indeed, this river is not called Lesien, no doubt its Chinese name, but is called Main Ilmainkhiaun, or perhaps rather Main Ma, from its passing a city of the latter name before it reaches the capital, from whence it proceeds to Tunquin.

Between the general map of the Mranma empire and this particular map of the Tarout Shan, although both drawn by the same person, there is an essential difference. In the general map, the country of the Tarout Shan is represented as extending to the bank of the Mækhaun River, while in the map now under consideration, a large space intervenes between that great river and the western boundary of the country represented. Perhaps it may be thought a sufficient excuse for this, that in a general map, on a small scale, much accuracy was not required; and the draughtsman, therefore, contented himself with delineating the countries of the Tarout Shan and Lowa Shan, as separated merely by the Mækhaun River; but when he came to delineate these countries on a larger scale, more accuracy became necessary, and he then delineated only the countries actually under the authority of the two tributary princes, while he reserved from each a large portion, at that time placed under the great Mranma officer stationed at Main Khain, to overawe the tributaries. Main Khain is situate on the west side of the Mækhaun, and no doubt the largest portion of the territory granted to this Mranma general was on that side of the river; but from what is represented in the map now under consideration, I think it highly probable that his grant extended also a considerable way beyond its eastern bank.

The extensive territory occupied by independent Lowa or Lawa, mentioned in my account of the general map (No. IV. p. 269. of this Journal), as placed at the sources of the river of Siam, would, from this map, appear to extend across the Mækhaun, and to occupy a space there on the N. W. frontier of the Tarout Shan, reaching almost to the bank of the Lesien or main Ilmain River. These Lowas are no doubt part of those rude tribes, that, under the name Lolo, have retained a kind of independence in the province of Yunan, notwithstanding the long-continued exertions of the Chinese empire.

Along the southern frontier of the Tarout Shan, extending from China all the way between the Tarout Shan on one hand, and Tunquin and the Lænzæn Shan or Lower Laos on the other, this map places another branch of the wild Lawas, which occupies a territory ten days journey in extent from north to south. This extent can only be considered as so wide in some parts. Between Tunquin and the Tarout Shan it must be narrow; but towards the Lænzæn Shan there is ample room for the extent mentioned. The village at the Customhouse (Kæn Rua), in that direction is mentioned in this map as ten days journey from the capital, while in a map of the Lænzæn Shan in my possession, Main Laun, the town of that country farthest up the Mækhaun, is stated to be seven days' journey from Sandapuri the capital of Lower Laos; but in another map in my possession, the whole distance between Sandapuri and Kiainroungri, the capital of the Tarout Shan, is a month's journey, leaving thirteen days' journey between these extreme towns, and thus affording ample room for the wild Lawas, even supposing the cultivation to extend a considerable way beyond Main Laun and the Custom-house, as it probably does. Concerning these Lowas I have already given it as my opinion, (No. IV. p. 268. of this Journal), that they are the aborigines of all the country now occupied by the nations comprehended by the Mranmas under the name of Shan, and who of course occupied originally by far the greater part of India beyond the Ganges, as well as of Yunan. This territory around the Tarout Shan, both on the north and south, seems, h[illegible] to be the largest space where they retain independence [illegible] (*Universal History*, vol. vii. p. 425.) of Siampa, [illegible]r tribe, have been long tributary to Cochin China, and their sea-coast at least is occupied by colonies from the latter country. It is probable, when the authors followed by the *Edinburgh Gazetteer* wrote their account of the Laktho, that the whole country now occupied by the Tarout Shan was occupied by these Lowa, who live in a state similar to what these authors describe as that of the Laktho. A colony of civilized Shan, probably the Kio to Tunquin, having since settled among the Lowas or Lolos, has civilized a considerable portion, and founded Kiainroungri and its dependencies.

The name of the Tarout Shan capital was written in the Mranma character by the relation to the secretary of state, whom I have already mentioned, (No. VII. p. 76. of this Journal). According to his orthography it should be Kiainroungri; and this I look upon as the best authority; but others call it Kiainiunghari and Kæniunghari, which look more different in writing than they appear when pronounced, the R by the Mranmas being generally softened in pronunciation to I or Y.

Art. XII.—*Geological Remarks on the Rock of Gibraltar and the adjacent Country.* By Mr John Baird *.

THE Rock of Gibraltar is a huge insulated mass of limestone, surrounded on three sides by the sea, and on the fourth by a low sandy tract of land called the Neutral Ground, by which it is connected with the continent of Spain. It is probable, I think, that this low neck of land, which in general rises but a few feet above the level of the bay, has at one time been covered by the sea, leaving the Rock of Gibraltar an abrupt rocky island mass a few miles from the main land of Spain.

The north and east sides of this rock present an almost perpendicular steepness from top to bottom. The west side slopes at about an average angle of 45°. The south end or side of the rock is at first quite perpendicular, and then falls gradually down towards Europa Point. The town is built near the foot of the west side of the rock. The length of the rock from N. to S. may be about 2½ miles; its breadth from W. to E. from half a mile to above a mile; and its height about 1000 feet above the level of the sea. The top of the rock is a long narrow ridge, running N. and S., the west side sloping down to the town and bay; the east side, from its rugged, perpendicular front, almost inducing the opinion, that Gibraltar Rock, as it now exists, is only the half of a large hill, the east side of which, in some great convulsion of nature, has been torn asunder from the other, and precipitated into the Mediterranean.

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* Read before the Wernerian Natural History Society.

The view from the top of the Rock of Gibraltar, the Mount Calpe of old, in a clear day, is most magnificent. To the east, the Mediterranean stretches out before us as far as the eye can reach; and on either side its lofty shores, the mountainous coast of Africa on the one hand, and, on the other, the more beautiful perhaps, but scarcely less hilly coast of Europe, both gradually receding from each other, to form, as it were, a broader basin for the Mediterranean; the village of St Roche, to the north, beautifully situate on the top of a gently sloping hill; the Bay of Gibraltar, and town of Algeziras to the west, and to the south the sister pillar, the lofty Mount Abyla, and her neighbouring mountains.

The Rock of Gibraltar is composed of limestone, of which there are two principal varieties, one, forming the great mass of the hill, hard, fine-grained, with a splintery or conchoidal fracture, possessing considerable lustre, and generally of a light-grey colour, sometimes also dark, sometimes nearly white, and in one part of the hill, where it is quarried as a marble, occurring beautifully variegated. This limestone is stratified, and near the top of the hill, as is well seen, the strata run from nearly N.E. to S.W., and inclining to the S.W. at an angle of 60° or 70°. The other principal variety is a conglomerate or brecciated limestone, formed of the debris of the former, connected by a red calcareous basis, and wrapping round the other central mass. This conglomerate variety appears to be still forming on the hill. Besides these, there occur two beds of a flinty slate rock, both very muc [illegible] and one of them containing numerous round and a [illegible] f limestone. These beds appeared to be contained in the older solid limestone, and to run in strata conformable to it.

Ample opportunities are afforded to gratify every wish of the geologist, in ascertaining the structure of the hill, by visiting the extensive excavations in various parts of the rock, which have been formed for the purpose of strengthening this great and important fortress. At the foot of the hill, the sole rock visible is the conglomerate limestone, which occurs in great abundance, and forming small hills. The imbedded masses are often of a very large size. The basis is a red, coarse, calcareous cement, or a calcareous tuff, more or less hard, and often intermixed with round concretions of calcareous sinter. At the foot of the hill the rock is often almost entirely composed of this calcareous tuff. As we ascend the hill, this conglomerate rock decreases in quantity, the imbedded masses become smaller, and the connecting basis less abundant, more compact, finer, and of a lighter colour. The imbedded masses, which are of every shape, are undoubtedly broken portions of the solid limestone nucleus. When we have ascended above two-thirds of the hill, this conglomerate encrusts the interior mass to the depth only of a few inches, and a little higher up almost entirely disappears, when the solid limestone forms the whole upper part of the hill.

That such is the structure of Gibraltar Rock, a central mass of old and solid limestone, covered to various depths by a newly formed conglomerate, such as has been described, appears, from the examination of those parts of the hill through which roads have been cut in the rock, of those long arches cut through both the conglomerate and solid limestone, and in particular of those amazing excavations, as they are called, planted with cannon, often running to a great extent, and parallel to the exterior surface of the hill, from which they extend into the rock from 20 to 50 feet, cutting in various places through the conglomerate into the solid mass. Partly owing to the darkness in these long arches, and from other circumstances, I seldom could discover any well defined line of separation between the solid and conglomerate limestones, though such a separation certainly exists. The imbedded masses of the conglomerate, however, the nearer they approached the solid rock, lay closer together, the interstices only between them being filled with the tufaceous basis.

That this conglomerate limestone is a much later formation than the interior mass, appears from the well-known and interesting fact of bones and teeth of large quadrupeds having been discovered imbedded in it.

The solid limestone, from the occurrence of beds of flinty slate in it, would appear to be a transition, or a very old secondary limestone, and it is extremely improbable, that organic remains of animals, so high in the scale of being, should be found in it, or in any rock contemporaneous with it. I have seen some of these petrified bones of large animals, and they were always imbedded in the conglomerate rock. Shells also occur in it, but always land-shells, and similar to the common species on the hill. These shells often are not at all altered. Some parts of this conglomerate are certainly of later formation than other parts, and I have no doubt that though slowly, its formation is daily going on.

There is a considerable depth of soil on some parts of the hill, in particular near its foot. It is a red, calcareous kind of soil, formed in a great measure from the mouldering of the calc-tuff; and appears to be very productive.

It is highly improbable, I think, that this calc-tuff basis has been deposited over the debris of the solid rock, from water which at any former period stood over the hill. Though I do not recollect that springs are very numerous on the hill, yet I think that it is far more probable that this calc-tuff has been deposited from such springs, as is usual with this substance in other situations, or from water percolating through the sides of the hill, and supplied, if necessary, from some great central reservoir, which I believe is not at all an uncommon circumstance in limestone rocks. The calc-tuff thus formed, and enclosing the debris of the solid limestone with animal remains of various kinds, explain the structure and nature of this rock and its petrifactions. It is probable, therefore, that petrifactions of animals of the latest formation, or even of man himself, may be discovered in t [illegible] lar rocks.

Numerous cav [illegible] e limestone, the sides, roofs, and floors of wh [illegible] ith a thick coating of calc-sinter, with numerous stalactites from the roof, and thick, massive pillars, as usual in caves of this kind. Many caves formerly existed, which are now entirely filled with calc-sinter and calc-tuff. Few of these caves are large. St Michael's Cave, about 800 feet above the level of the sea, which is the largest and best known, is about 100 feet in length, 40 in breadth, and 40 or 50 in height. The stalactites are short and thick, and generally of a brown calc-sinter, which is heavier and harder than the other varieties. At the farther extremity of this cave are many deep hollows; in some of these I found parts of the skeletons of goats, which had no doubt fallen into these pits, and being unable to escape, had there perished. Bones and skeletons may in this way be often found in these caves, encrusted and petrified by the calc-sinter. Below this upper cave occurs another smaller cave, but more beautiful, into which you descend by rope-ladders by one of these deep hollows. Many amusing fables relate to this cave. Hundreds of small caves occur in the rock, generally I think situate in the conglomerate, and filled in part with calc-sinter and calc-tuff. St Michael's cave, however, occurs in the solid limestone. Some of these caves present most picturesque and magnificent appearances. The calc-sinter of these caves, and the calc-tuff of the conglomerate limestone appear to have a similar formation; the tuff is associated with the sinter in the caves, the sinter is associated with the tuff in the rock: if the one, therefore, is gradually forming, so is the other; if the one is formed by percolation of water through the rock, which holds the calcareous matter in solution, so is the other; and thus the formation of the one is connected with, and illustrates the formation of the other.

To the N.E. of the Rock of Gibraltar, about 15 miles on the shores of the Mediterranean, rises a very lofty range, called the Apluxara Mountains, steep, massive, and bare. I did not examine these hills, but they are probably a continuation of the limestone of Gibraltar. The neutral ground which connects Gibraltar with Spain is 2 or 3 miles in length, beyond which the country rises into round, sloping hills. The rocks, to the distance of 10 or 12 miles to the N. W. of Gibraltar, are various kinds of limestone, coarser than the limestone of the

Rock, and resting upon it. They are stratified, the strata running N. E. and S. W. nearly. At the foot of a range of pretty high hills, behind, or rather N.N. W. of Algeziras, and 10 miles N. W. from Gibraltar, I found some masses of a large granular red and white sandstone, very like the old red sandstone. These masses increased in number as I ascended the hills; but as I did not proceed to the top, I did not discover the rock *in situ*.

From the circumstance of the plague raging in different parts of Spain, at no great distance from Gibraltar, at the time of my visit to this place, no person was allowed to be absent from Gibraltar longer than 24 hours, a time too short to admit of an accurate or extensive examination of this part of the country.

EDINBURGH,
May 1822.

ART. XIII.—*List of the Steam-Boats plying on the River Clyde in* 1822. In a Letter from JAMES SMITH, Esq. F. R. S. E. to Dr BREWSTER.

MY DEAR SIR,

AGREEABLY to your desire, I send you a list of the steam-vessels at present plying upon the River Clyde. The whole of them, except the Liverpool boats, sail from Glasgow to the ports specified in the 2d column; the Liverpool boats go no farther up the river than Greenock. The 3d column contains the number of horse-power of the engines; the 4th column the tonnage, from which the space occupied by the machinery is deducted; and the last column contains the tonnage, calculated in the same manner as other ships. The boats which ply between Glasgow and Greenock generally make two, and frequently three trips a day; and hitherto not a single passenger has lost his life, either from sea-risk, or the nature of the machinery. I am, my Dear Sir, yours always,

JA^{s}. SMITH.

JORDANHILL,
14*th May* 182

LIST of Steam-Boats Plying on the River Clyde in 1822.

	Port.	Power of Engine.	Tonnage, exclusive of Engine.	Tonnage, including Engine.
Superb,	Liverpool,	120	160	240
Majestic,	do.	140	239	345
City of Glasgow,	do.	120		
——— building,	do.			
Eclipse,	Belfast,	60		
Britannia,	Campbelton,	32	69	109
Argyle,	do.	26	66	78
Highlander,	Loch Sunart,	24		
Comet,	Fort William,	30	29	
Highland Chieftain,	Stranraer,	16	37	65
Inverary Castle,	Inverary,	40	71	112
Rothsay Castle,	do.	30	61	95
Towart,	do.	50		
Neptune,	Rothsay,	20	58	82
Highland Lad,	Lochgoil-head,	12	34	51
Fingal,	do.	16	52	67
Port-Glasgow,	Helensburgh,	16	55	84
Greenock,	do.	10	38	52
Caledonia,	do.	30	57	
Waterloo,	do.	20	58	90
Glasgow,	Largs,	16	43	64
Albion,	do.	20	63	94
Largs,	do.	35		
Dunbarton,	Dunbarton,	16		
Marion,	Loch Lomond,	20		57
Postboy,	Greenock,	20	54	
Gourock,	do.	14	45	65
Marquis of Bute,	do.	14	36	53
Robert Burns,	do.	20	48	68
Oscar,	do.	12	37	51
Trusty,	Goods,	10	59	88
Industry,	do.	10	55	79
Dispatch,	do.	10	58	83
Active,	do.	10	59	83
Sampson,	Towing-vessel,	40	49	
Hercules,	do.	60		130

Art. XIV.—*Descriptions of two new Genera of Nepaul Plants.* By Mr David Don, Curator of the Lambertian Herbarium*.

A species belonging to one of the new genera now to be described, has been published long ago by Roxburgh †, under the name of *Incarvillea parasitica.* It is surprising that so acute an observer should have referred his plant to *Incarvillea,* a genus with which it scarcely agrees in one single point. The genus in question, which I propose to call *Trichosporum,* is remarkable for the singular structure of its capsule and seeds. The capsule is a span or even a foot long, ensiform, compressed, 4-celled, 2-valved; when ripe, the valves separate from each other, along their whole length: the septum finally bursts at the margin, continuing, however, always firmly fixed at its middle; the sides then become erect, and embrace each other. The seeds are very numerous, oblong, cylindrical, and rough, with minute warts; emitting at the base one and apex two very long silky hairs. These hairs are consequently so numerous, that they completely cover the seeds, as in plants of the Order *Asclepiadeæ.* *Lysionotus,* the other genus, agrees in many respects with *Trichosporum,* but differs in the two-celled valves of the capsule bursting at the suture on the back; the seeds being smooth, and attenuated at both ends into a caudiform capillary appendage; and in having a 5-leaved calyx, and also in habit. The genus *Didymocarpus* of Wallich comes very near, both in habit and structure, to *Lysionotus;* but is advantageously distinguished from it, by its smooth ovate seeds, destitute of any appendage, and by its monophyllous 5-toothed calyx. These three genera form a small family of plants, to which the name of *Didymocarpeæ* may be given. They differ essentially from the *Bignoniaceæ,* as limited by Mr Brown, to which they are nearly related, in having a simple clavate stigma, pendulous, minute, ovate or cylindrical, frequently appendaged seeds; in the structure of their capsule, and form of their embryo. The plants which compose this little family, are all natives of Nepaul and the East Indies, and are mostly dwarfish herbaceous perennial plants. The genus *Chirita* of Dr Francis Hamilton, which includes the *Didymocarpus speciosa* and *bifolia* of Wallich, has a 5-parted calyx, and a bifid stigma. They are small herbaceous perennials, with simple stems; opposite serrated or crenated leaves, and solitary peduncled, axillary flowers. Their fruit, however, and consequently the structure of their seeds, is unknown to me; but their habit, and having a divided stigma, would incline me to place them among the *Scrophularinæ*, rather than in this order.—I shall now proceed to give descriptions of *Trichosporum* and *Lysionotus*, as well as the character of the family to which they belong.

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* Read before the [redacted] 6th January 1822.

† *Plant. Coromand*

Nat. Ord. DIDYMOCARPEÆ.

Calyx campanulatus, 5-fidus rariùs 5-phyllus, æqualis.

Corolla basi tubulosa, supernè ampliata, campanulata: *limbus* 5-lobus, sub-bilabiatus: *lobis* rotundatis, in æstivatione imbricatis.

Stamina 4 didynama, quorum 2 in *Didymocarpo* sterilia: *filamenta* glabra, capillaria, fornicata: *antheræ* ovales v. oblongæ, nudæ, biloculares, per paria parallelo-adnatæ, in *Trichosporo* apice tantùm conniventes.

Pistillum 1: *germen* ensiforme: *stylus* filiformis: *stigma* simplex, clavatum sæpiùs excavatum, subinfundibuliforme.

Capsula siliquosa, compressa, bivalvis, 4-locularis, *dissepimento* duplici, membranaceo: *valvis* primò discedentibus: *loculis* usquè gemmatis tardiùs ad commissuras dorsales v. marginales dehiscentibus.

Semina ∞ minuta, pendula, ovata v. cylindracea, nuda aut rariùs in caudam brevem, capillarem utrinque desinentia v. pilos longissimos emittentia, glabra rarissimè scabra: *albumen* nullum.

Embryo minutus, teres, inversus: *cotyledones* oblongi embryone plùs duplò breviores: *radicula* superior, cylindracea.

Herbæ *perennnes, terrestres, acaules rariùs caulescentes v.* frutices *parasitici,* foliis *oppositis integerrimis coriaceis glabris: in* herbis, folia *plerumque radicalia, cordata, crenata v. serrata, hirsuta, in* caulescentes *terna.* Flores *umbellati, pur-*

purei, rosei v. rubri ; in herbis, umbellæ *dichotomæ, basi involucro et involucello diphyllo munitæ.*

TRICHOSPORUM *.

INCARVILLEÆ species, *Roxburgh.*

Calyx campanulatus, 5-fidus, ebracteatus : *segmentis* æqualibus. *Corolla* tubulosa, incurva, basi angustata, supernè ampliata : *ore* obliquo, 5-lobo : *lobis* plerumque rotundatis, subæqualibus in æstivatione imbricatis. *Stamina* 4, longè exserta, fertilia, 2 quorum parùm breviora : *filamenta* capillaria, fornicata, glabra : *antheræ* ovales v. oblongæ, nudæ, biloculares, obtusæ v. subacutæ, basi fissæ, apice per paria conniventes, internè post dehiscentiam lamellatæ. *Pistillum* 1 rectum : *germen* compressum, ensiforme, sensìm desinens in stylum filiformem : *stigma* simplex, clavatum disco excavato infundibulari, ore integerrimo. *Capsula* siliquæformis, compressa, (palmaris pedalisve), pendula, ensiformis, acuminata, bivalvis, 4-locularis dissepimento duplici : *valvis* planis per totam longitudinem primò discedentibus, extus lævibus intus lacunoso-rugosis : *septa* tenuia, papyracea, per medium arctissimè affixa, demùm ad oras dehiscentia, postmodo lateribus inflexo-approximatis. *Semina* ∞ oblongo-cylindracea pendula, ad septa imbricatìm affixa, papillis minutissimis scabra, ad baseos 1 apices 2 pilos longissimos tenuissimos sericos emittentia, inter quos occulta.

Frutices *parasitici.* Caules *simplices, teretes, geniculati, glabri, secus truncos arborum in more* Epidendrorum *scandunt et ad geniculos fibras simplices subsolitarias sæpiùs enascuntur.* Folia *opposita, petiolata, lanceolata, acùminata, carnosa, integerrima, glabra.* Flores *terminales umbellati.* Pedicelli *breves, uniflori basi unibracteolati.* Corollæ *rubræ, extus pilosæ figurâ ferè omninò* Digitalis. *Toto cælo diversa est* Incarvillea *Juss. cui* calyx *tribracteatus,* antheræ *bilobæ,* stigma *bilamellatum,* capsula *bivalvis bilocularis,* dissepimentum *simplex valvis oppositum,* semina *plana, suborbiculata membranâ latiusculâ cincta ;* caulis *erectus ramosus herbaceus,* foliis *laciniato-pinnatis*

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* Named *Trichosp* [redacted] *ιχος, hair ;* and *σπορα, seed,* on account of the seeds being furn

ramisque alternis, floribus *laxè spicatis*, in characteribus omninò cum *Bignoniaceis* convenit, inter quas rectè ordinanda, tamen habitu discrepat quod magis ad *Scrophularinas* referrens ab his defectu albuminis differt.

1. T. *grandiflorum*, foliis elongato-lanceolatis acuminatis enerviis, umbellâ multiflorâ, laciniis calycinis ovatis, corollæ lobis omnibus rutundatis subæqualibus, stylo exserto.
    Incarvillea parasitica, *Roxb. Coromand.* t. 291.
    Crescit supra arbores Montium propè Sylhet, Indiæ Orientalis.—*Wallich.* ♄ ◡.
2. T. *parviflorum*, foliis ellipticis acuminatis nervosis, umbellâ pauciflorâ, laciniis calycinis linearibus, corollæ lobis superioribus brevioribus acutiusculis, stylo incluso.
    Crescit secus truncos arborum in montosis Nepaliæ *inferioris. Wallich.* ♄ ◡.

Species à præcedente distinctissima: floribus triplò minoribus, laciniis calycis linearibus, lobis corollæ 2 superioribus multò brevioribus acutiusculis, foliis duplò latioribus brevioribusque nervosis.

These two species, although perfectly distinct, have been sent in the recent collections from India indiscriminately under the name of *Incarvillea parasitica*, Roxb. Both species are parasitical shrubs, with showy flowers, resembling in habit some species of *Loranthus*. The flowers of *T. grandiflorum* are not inferior to those of *Digitalis purpurea.*

LYSIONOTUS *.

INCARVILLEÆ sp. *Wallich.*

Calyx 5-phyllus: *foliolis* æqualibus. *Corolla*.................. *Stamina*... ....*Pistillum* 1: *germen* ensiforme, basi attenuatum, desinens in *stylum* filiformem: *stigma* simplex, clavatum; *disco* excavato ore integerrimo. *Capsula* siliquosa, compressa, (triuncialis) pedicellata, stylo persistente acuminata, bivalvis, 4-locularis: *dissepimentum* duplex, membranaceum: *valvis* medium discedentibus: *loculis* usque gemmatis, demùm e suturâ dorsali dissilientibus, extus lævibus, intus lacunosis, seminiferis. *Semina* ∞ oblongo-cylindracea, pendula, lævia utrinque incaudam setiformem ipsa duplò excedentem desinentia.

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* The name is derived from *λύσις, εως, separation, dissolution;* and *νῶτος behind, the back;* from the cells of the capsule bursting at the back,

Herba *perennis, terrestris.* Radix *repens, crassitie pennæ anserinæ, superne lignosa cortice fuscâ tecta, fibris capillaribus plumosis numerosis instructa.* Caulis *erectus, palmaris, cylinlindraceus, glaber, lævis, subcarnosus.* Folia *terna, ovato-lanceolata, acuminata, coriacea, serrata, petiolata, utrinque glabra, viridia, reticulatìm venosa.* Petioli *brevissimi, semiteretes, glabri, supra canaliculati.* Umbellæ 5—10-*floræ, pedunculatæ.* Pedunculi *erecti, filiformes glabri, 3—4-pollicares, terni, parùm infra apicem caulis siti axillares simulantes.* Pedicelli *teretes, glabri, basi* involucellis *duobus subrotundo-ovatis muniti, sæpiùs simplices, nudi, 1-flori rariùs divisi medio bibracteolati, in* Didymocarpo *proximo* calyx *campanulatus 5-dentatus,* semina *ovata nuda non appendiculata.*

L. serrata.

Crescit in Nepaliâ. *Wallich.* ♃.

In the Lambertian Herbarium is a single specimen of this plant, lately sent by Dr Wallich, the indefatigable superintendant of the Calcutta Botanic Garden, with the name *Incarvilleæ species* attached to it. The specimen, otherwise very perfect, is without flowers; but from the great affinity it bears to *Didymocarpus*, it is very probable that it agrees also with it in the form of its flowers, as well as in the disposition and figure of its anthers *.

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* Since the publication of my little paper on ***Rhododendron***, &c. in the *Phil. Journn.* (vol. vi. p. 47.) a friend has pointed out to me, that the union of the genus ***Azalea*** (excluding ***Azalea procumbens***) with ***Rhododendron***, has been long ago proposed by R. A. Salisbury, Esq. in his observations on ***Rhododendron chrysanthum***, in the late periodical work entitled "Paradisus Londinensis." I take this opportunity of mentioning, that the following improvements and corrections may be made on the character of ***Rhododendron***: P. 49. for "basi hirsuta." read "sæpiùs basi hirsuta." After "5-valvis," insert "(in *R. arboreo* 10-locularis, 10-valvis)." For "apice dehiscens," read [redacted] dehiscens." The columella should have been described "columella [redacted] *orco* 10-angula)."

ART. XV.—*Notice of a species of Cannibalism, practised in the interior of Sumatra, together with some particulars relative to the Customs of the Inhabitants, and the produce of the Country about Tapanooly, including the Camphor Tree.* Communicated in a Letter from India *.

1. *Cannibalism, and Customs of the Battas.*

AS the Helen is still in company, I sit down to fulfil my promise of an account of Tappanooly, and the Battas who inhabit the interior of that part of Sumatra. They had been stated to be *cannibals*, and we were curious to ascertain the fact, and learn something of so peculiar a state of society. We therefore assembled some of the most intelligent chiefs, whom we examined at length respecting all their usages and customs, and obtained the most ample and indisputable information on every point. The history of these people is so extraordinary and peculiar, that I should not have credited it on any evidence less than that which we received, and which I should almost fear to communicate, were I less convinced of its absolute correctness.

That they are *cannibals* is placed beyond a doubt; but the circumstances and manner in which it is practised, are, I believe, unexampled in the history of the human race. The eating of men is not merely practised in war, as in some other savage countries, but is the punishment solemnly and deliberately awarded by their laws for certain capital crimes. Five cases are enumerated in which the eating of offenders is ordained, of which the first, and, in their ideas the greatest, is adultery. The sentence is passed in full council by the assembled chiefs, and is publicly carried into effect three days after, when the whole neighbourhood is assembled. The victim is tied up, with his hands extended, and the injured party is asked what part he chooses; he perhaps desires the ears; they are instantly cut off, and he deliberately eats them, either raw, with limes and pepper, or dressed, as he pleases. Every person present then cuts off and eats what part he likes; and after all are satisfied, the chief enemy cuts off the head, and carries it home, to suspend it in triumph on the top of his house. Thus the culprit is literally eaten alive, and with a coolness and deliberation, that I believe to be absolutely unparalleled. You will have difficulty, I know, in believing this, but I tell it you plainly, according to the information we received from the people themselves, who seemed to think very little of it. Such severities of punishment must of course operate to make a crime of rare occurrence; and another check to its frequency is, that the injured party may, if he pleases, commute the punishment for a pecuniary compensation, which avarice often tempts them to do. In short, it seems to be like Shylock's pound of flesh, an atonement the party aggrieved has a right to, and which he may dispense with if he pleases.

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* Read before the Wernerian Society, 18th May 1822.

The Battas are evidently of Hindoo origin, and these customs afford another example of the *mild* spirit of that religion, which denounces damnation on the slayer of a cow or an ant, yet makes its sport of human life, and of every affection in our nature. Formerly it was their practice to eat their parents, when they became too old to be useful, but they say that latterly it has been abandoned. Now, you will of course suppose, that these people are in the lowest state of barbarism to which nature can be reduced; but, strange inconsistency! it is quite the reverse, and they have even many noble and estimable qualities.

In point of veracity and sense of honour, they are as much superior to the Bengalees, as we are to both. Their deportment and behaviour is manly and independent. In some things their notions are carried to a most extravagant length: a man, for instance, must not marry a woman of his own tribe, but must seek a wife in some other tribe, that acknowledges different ancestors. The breach of this rule is punishable with eating. This is carrying the idea of consanguinity much farther than we do. If two men quarrel, and their difference cannot be accommodated by mediation, they go to war; but before commencing operations, they must publicly proclaim the war in the fairs, that the other may have proper warning. If a man should kill another without this public proclamation of hostility, he would be sentenced to be eaten; but after it, such an act is legal. Even then, however, being only a private quarrel, he is not permitted to eat his e[illegible]ough he may kill him. It is only on great occasions, w[illegible] nation goes to war, that eating enemies is permitt[illegible], it is a point of honour, that no violence or treachery be committed: a man who carries his musket to the fair, sticks a green branch in the muzzle of it, in proof of peaceable intentions.

The Battas have a written character peculiar to themselves, and books on various subjects: we have got an account of five or six. The country in the interior is populous, and well cultivated, and farther, abounds in gold. Camphor and Bayamin are the wild products of their forests, and are procured in no other part of the world. Thus, few countries surpass it in natural riches. The people in the interior have an aversion to the sight of the sea, thinking it the abode of evil spirits; and the inhabitants of the coast are in consequence of this an inferior race. They acknowledge one Supreme God, and three inferior divinities. Their names shew them to be of Hindoo origin, as well as the title of their greatest chief, *Sa Singa Màha Rajah*, which is pure Sanscrit.

So extraordinary a people would require to be better known, and we shall probably sooner or later make an expedition into their country. This will be very practicable, as the chief of Baroos, one of our friends, has lately married the daughter of a Batta Chief. I should have mentioned that women are excluded from their human feasts. Who knows but we may yet civilize and reclaim these people; I think they have sterling good qualities that would make it worth while. At all events I should like to get among them, and have ocular proof of their customs; it may be we shall yet be present at a human feast. We told the chiefs we were anxious to partake, and asked them what were the epicurean morsels. They laughed, but said the palms of the hands, and soles of the feet, were the pieces most prized.

2. *Account of the Camphor-tree of Sumatra.*

The Harbour of Tappanooly is a most noble and extensive one. The hills come down to its edge, and are clothed with luxuriant forests of Camphor, &c. One settlement is on a very small island in the middle of it, most romantically situated, where there is a small fort, two or three houses for the Resident and his assistants, and a small bazar of perhaps three or four hundred people. The population around is very scanty, and their villages are situated in the hollow of the hills, where they lie hid till you come close upon them.

The *Camphor trees* are the monarchs of the forest, being often a hundred feet perpendicular to the first branch; and they are as straight as masts. We ordered one to be cut down, and got a little camphor in it. The camphor is found in concrete masses, in cracks and hollows in the heart of the tree. Very little of it finds its way to Europe; it chiefly goes to China, where it bears a price about thirty times that of the China camphor, which is the article we use. The latter is the produce of the *Laurus Camphora*, obtained by boiling; the former of an imperfectly known genus, called by Gaertner, who only saw the fruit, *Pyrobalanops*, and is the natural product of the tree. It is not exactly known what occasions its enormous value in China, 3000 dollars a pecul of 133 lbs. It has been supposed that it was mixed with their own camphor, and sold again in that adulterated state; but the difference of price renders this improbable. I rather suspect that the Chinese, whose epicurism is very extraordinary and different from ours, use it in some way or other for culinary purposes. Besides the camphor, the tree yields an oil which is very powerful. It only flowers once in four or five years, and was not in flower when I was there. I got, however, specimens last year. I scrambled over several of the hills during the two days we remained at Tappahooly, and got some new plants. * * * * Here there is in fact a field new and untrodden by the foot of science, a harvest reserved for me to reap; and it shall not be neglected, for I have every advantage and opportunity. * * * *

INDIANA, OFF NATRALL,
29*th February* 1820.

ART. XVI.—*Remarks on the Larus Parasiticus or Arctic Gull; and on the Larus Rissa or Kittiwake; with an Account of the Greenland Kittiwake;—and on the Colymbus Grylle**.

By LAWRENCE EDMONDSTON, Esq. Zetland.

THE materials of a system of zoology, established on a broad and permanent fou may be expected to be chiefly derived from the coll f individuals describing the species of their respective countries; and had the value of this principle been sufficiently appreciated, much erroneous, though ingenious, speculation might have been precluded. Influenced by this impression, I have occasionally had the honour of submitting to the Society observations on some species of birds, of which the history and distinctions seemed not sufficiently ascertained; and, on the present occasion, I shall take the liberty of again trespassing, with similar views, on its attention.

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* Read before the W d 18th May 1822.

Although I am not aware of the circumstance, it is possible that some of the following remarks may have been anticipated by other ornithologists; but as they have been derived from long and personal observation of the birds to which they relate, in their native situations, and originally quite independently of books or museums, they may have their use, should this consist only in confirming the views of abler naturalists; for facts are sometimes more valuable in their confirmation than discovery.

1. *Larus Parasiticus, or Arctic Gull.*

The first species I shall notice is the *Larus parasiticus*, or *Arctic Gull*, regarding which much diversity of opinion has existed; some authors considering the white-breasted Arctic gull as a species distinct from the brown, while others maintain it, with equal confidence, to be the male, the latter being the female,—that, therefore, the difference is merely that of sex. This opinion, however, I have unequivocally proved to be erroneous, by careful dissection. Of four of the white-breasted birds, which I opened in Zetland, in the month of August last, one was most clearly a *female* full of eggs, the others were males; of an equal number of the brown individuals, all were distinctly *males*; and before this period I had met with several females in the same plumage. Their difference, then, as depending on sex, is disposed of. It must therefore be considered as connected either with difference of species, or progressive changes of plumage of the same species, for it is too general and regular to be referred to mere variety.

In the size, general appearance, form, modes of life, habits of migration, and anatomical structure, these two birds perfectly coincide; and this agreement has not, I believe, been materially denied, even by those who contend for their distinction. In what, then, can they differ, except in plumage? Yet I fancy it will not be assumed, that this alone is sufficient to establish accurate distinctions, in opposition to the other circumstances which I have enumerated, in which they thoroughly correspond. But if it shall be found that even this distinction is nugatory—if the two birds shall occur in characters of plumage insensibly passing into each other, then, I apprehend, we should have sufficient evidence of the accuracy of the opinion which I am disposed to maintain, That they are the same species, differing only in age; the white-breasted Arctic gull being the species in its adult dress, the brown individual the same in the young and imperfect state. In this opinion I am happy to find I am supported by the authority of Montagu, whose industry and zeal have so much contributed to elucidate British ornithology.

I have procured specimens of the Arctic gull, in shades of plumage intermediate between its young, usually called the Black-toed Gull, and the perfect white-breasted adult bird; some of the former passing into the characters of the brown bird, and these again acquiring the adult appearance.

They breed in the same situations, and promiscuously; and single pairs, sometimes of two brown, or two white-breasted ones, or of a brown and white, with their nest and eggs, are often found on one separate hill, so far removed from other haunts, more numerously occupied by their species, as to render this proof little subject to suspicion. This remark of their promiscuous pairing will also be strikingly confirmed by traversing the heaths where they are most frequent. But this habit, though strongly corroborating the opinion I am supporting, is not essential to its establishment; even the opposite fact might very naturally be reconciled to its accuracy. A more indirect objection may perhaps occur, from the generally correct analogy of the other gulls, which are not observed to breed till they have attained to permanent and perfect plumage. It will be remembered, however, that this bird, and the Skua gull, are so strikingly distinct in their general aspect, and in many of their habits from the gull tribe, that they have been very properly formed, by later [illegible]ists, into a separate genus (*Lestris*); and hence this appa[illegible]es not apply.

If they are separ[illegible]hould naturally expect to find some difference in their young, but no variety have I ever met with, farther than what is casually found to affect the young of any one well ascertained species of bird, some individuals being of darker, others of lighter shades of plumage.

On a heathy hill, in the Island of Unst, Zetland, which has long been a favourite resort of the Arctic gull, and where I remember, some years ago, to have seen more than fifty pair, I did not, last year, find above five. They have been observed to be gradually decreasing in number, from the hill being constantly exposed to the depredations of idlers in quest of their nests. The few that remained were all white-breasted, with one brown individual only. And this would seem another presumption for the opinion, that the brown is merely the Arctic gull in imperfect plumage,—for, from hardly any of the young being reared on this spot, and from the general fact, which I conceive nearly established, that certain families of migratory birds, and their descendants, regularly occupy certain situations, I should conclude, that, in this instance, only some of the old stock could have been able to remain.

During the breeding season, this species confines itself mostly to the heaths, feeding chiefly on the insects that frequent the marshes. Occasionally a straggler dashes along the shores, to the terror and annoyance of the Terns and lesser gulls. The kittiwake seems peculiarly the victim of its persecution; but indeed hardly any birds of its size are secure from the attacks of this aquatic hawk: when stimulated by hunger, it hesitates not to assault and harass them, to compel them to disgorge their food. I once witnessed a very animated and amusing chace of this kind, when the game was the common wild pigeon. This bird, which displays great agility and rapidity of flight, the Artic gull seemed as easily to overtake, as had a hawk been pursuing a heron. In the celerity of its flight, and the precision with which it hit its object, it far surpassed any hawk, and for which the terrified pigeon seemed to take it.

The joint of the wing projecting so as to form a kind of spine, its rapidity of flight, muscular energy, and bold disposition, must render it a very formidable opponent. It is not easily domesticated, unless taken very young, although so bold and familiar in its native state.

The voice is very similar to that of the kittiwake, but stronger, and not so well defined. Both these species follow nearly the same periods of migration, and, I suppose, visit the same latitudes. It is curious, however, that while some of the young of the kittiwake remain throughout the winter in Zetland, I know of no instance where an individual of the Arctic gull has been met with there during that season. That singular instinct, which seems to condemn it to depend for its chief supply of food on the superior industry or dexterity of other birds, irresistibly impelling it to follow the main body of those caterers for its subsistence, in their great periodical migrations.

2. *Larus Rissa, or Kittiwake.*

Of the multitudes of the *Larus rissa, or Kittiwake*, that annually arrive in Zetland to breed, large flocks are observed to keep apart from those which repair to the usual haunts for incubation, resting on the water, or on low rocks; and, from their not breeding, are termed, in the dialect of the country, *Yeeld* Kittiwakes. This singular fact in their history has been stated by Dr Edmondston, in his "View of the Zetland Islands," but I am not conscious that any explanation has yet been offered of it.

I regret that indisposition prevented me last season from procuring any specimens for dissection, since I suspect those *Yeeld* Kittiwakes to be merely the young of the first year, which, although attained to perfect plumage, have not yet acquired the faculty of propagation. Of all the individuals of this species which are met with in Zetland during the summer, I have never observed one that is not in the adult plumage; and it seems more reasonable to suppose, that so large a number have not attained the power of continuing their species, than that they have lost it; or that they are all of one sex.

If this opinion be correct, it will establish a fact, which it may be useful to keep in view in ornithological distinctions, but to which sufficient attention has not always been paid, that although a bird may be arrived at mature plumage, it does not necessarily follow that, in other respects, it is perfect.

I suspect this gene [illegible] k may apply to the three more familiarly known specie[illegible] and the Lesser black-backed, and the Herring G[illegible] ie summer, when the herrings, in their passage southward, swarm on the coasts of Zetland, numerous flocks of these three species of gull, in all possible stages of plumage, are found attending them. Many of these are in mature plumage, but are never observed to frequent the cliffs where their species breed, and are therefore also called *Yeeld.* I have opened a few of them, and their sexual appearances were very imperfect.

At a particular time of the tide the herrings descend from the surface, and, during the interval of their absence, these gulls generally repair inland, and rest amongst the heaths. As soon, however, as the tide changes, and the herrings again rise, they tumultuously quit their retreats, and, by their wild and discordant screams, express to each other the anticipated joy of their approaching feast. When they take wing, it is a sign that the period of fishing is at hand. I have often been astonished at the precision and regularity with which flocks, far removed from the view of the sea, seemed, from a state of tranquillity and repose, suddenly to display bustle and activity, and instantly bend their course to the fishing-ground. This has been supposed to depend on a certain change of the air, coincident with the change of tide, which, though not cognisable by our senses, may be evident to theirs: but such a change and such sensations seem quite hypothetical. I should more simply account for it on the supposition that other individuals of their own species, engaged in the same pursuit, and nearer the fishing-ground, had by their sounds or movements given them notice of what was going on.

Young birds, therefore, often differ from adult birds, not less in habits than in plumage, and often, when perfectly agreeing in the one, they essentially differ in the other;—remarks which, though sufficiently simple, and obviously useful, have been too frequently lost sight of in ornithological discussion.

The kittiwake, from its number and familiarity, affords also an excellent example for studying the general habits of gregarious and migratory birds. One remark, its history well illustrates, which, I believe, applies to all the larger and more regular migratory birds, that they not only return annually to the situations appropriated by their species for incubation, but also that only certain tribes of their species occupy certain parts of these situations, and that this right of property, as it were, is respected and maintained, as long as any of their descendants remain to enjoy it; and that not until, from the multiplication of the individuals of the tribe, their abodes become too circumscribed, or until they are forcibly dispossessed of them, do they go in quest of a new residence.

Amongst those families and species, where depredations can proceed only to a certain extent, and which have for some years reached their height, the numbers that annually arrive to breed are usually observed to be nearly the same; they are generally, I believe, greater the year succeeding one favourable for incubation, and when the young have been remarked to be very numerous.

With regard to those, on the contrary, that are hardly ever suffered to rear their young, progressive diminution annually takes place. Yet, although repeatedly disturbed, they do not quit the scenes of their former domestic happiness, but regularly, and ineffectually, attempt to renew it, till at length, each dropping off by age or accident, the family becomes extinct.

It is interesting to observe the few that remain, still lingering over their ancient haunts, even after their nests have been repeatedly pillaged, and the season for renewing them gone past, and zealously endeavouring to defend them from intruders, or by their restless movements, and anxious screams, reproaching those whom their threats or assaults were unable to expel.

If they were not under the operation of this powerful and imperative law of instinct, why should they pertinaciously cling to particular spots, where they are regularly persecuted and prevented from breeding, and not seek an asylum in other situations unoccupied and unmolested, or retire to comparatively protected ones, in the vicinity of individuals of their own species, with hardly a hill intervening? If I mistake not, this remark extends to herons, rooks, and black-headed gulls; and though some exceptions should exist, the general observation will have its advantage.

I have had occasion peculiarly to verify it, in what has befallen some migratory birds, that breed in situations easily accessible. The lesser n[illegible]nd terns, and oyster-catcher, for instance, that breed [illegible]l situations, near the shores; the curlew and Ar[illegible]eed on the heaths, have become extinct in some districts, where but a few years since they were numerous; nor is it observed that, when they decrease in one situation, they proportionally multiply in another. And should we not naturally suppose, that when molested in one quarter of a country, they should rather remove to another part of it, where they find their own species and congenerous birds undisturbed, than, from being persecuted in any particular part of a country, infer, if I may so express it, that they alone of their species, like the "dove of the deluge," have there no resting place, and emigrate to distant and unknown regions? Why may not instinct attach them to a distinct situation, as well as to a particular kind of elevation, or soil, or climate, or form of nest? It may be as difficult to change the impulse that determines certain families of birds to definite situations, as that which attaches their species to certain countries, or to a particular form of nest,—to make the arctic gull breed on the shelves of rocks, or the auks on the heaths. I do not assert that no addition is ever made to certain families of birds, but that these families, as long as any of them remain, always possess the haunts of their predecessors. And this seems a beneficent provision of Nature to preserve a due balance in the diffusion of each species, and to prevent the frequent quarrels and fatal contests among the feathered tribes, which the want of it would inevitably occasion.

It is, besides, inconsistent, first to assume that birds are mainly intended for the use of man, and then, Tantalus-like, to remove them from his reach the moment he wishes to avail himself of their advantage; but this would be the case, if it be not generally incorrect, that birds remove from those situations where they are molested; and if it be not incorrect, we ought, in exploring those retreats to which they are presumed to remove, to find a proportional increase in number.

It would, therefore, appear to be erroneous to suppose that birds withdraw to more retired haunts, as their enemies or civilisation increase; but more natural to conclude that they have been extirpated in certain situations which they once inhabited, and where they are now no longer to be found; and hence we may anticipate the extinction not only of certain tribes of the same species, but perhaps of certain species themselves, from the progressive increase of similar causes of extermination.

The kittiwake, like most of the gulls, is frequently on wing, and is a very industrious and expert fisher. Its usual mode of catching its prey is, like the gannet or the tern, to precipitate itself with sudden velocity from the air, several feet beneath the surface of the water.

It is familiar, and easily tamed; but, apparently from the delicacy of its constitution, it seldom lives long in confinement. It is subject to a disease very similar to the tubercular phthisis of the human species. After employing many unavailing efforts to save a favourite individual I had domesticated, and originally procured, by rescuing it from the insidious attacks of a cowardly raven, it continued to languish, and at length died quite emaciated. I opened it, curious to know the cause of its death, when I observed the lungs quite studded with tubercles, and ulceration surrounding them in every direction. We know how much physiology is indebted to comparative anatomy for its advancement; and a more extended and accurate investigation of the diseases of the lower animals, than they have yet received, might elucidate principles in general pathology, which are still so ambiguous and obscure.

This species lays two eggs, and its young is now generally admitted to be what was so long described as a distinct species, under the name of *Larus tridactylus*. Indeed, it is not a little singular that so glaring an error should have been only so recently removed. This is, however, but another example of the little accuracy in distinction which may be looked for, from the mere consideration of the external characters of birds. In these, it is true, the kittiwake and its young differ much; yet who could ever doubt they were the same species, after having seen them together in their native cliffs?

But although the *tridactylus* of most authors be without any question the young of the kittiwake, a bird has been described by other writers as the same, on which I confess some doubt may exist.

Last November I obtained several specimens, one of which was sent to the Wernerian Natural History Society, of a bird similar in many respects to [illegible]wake, but remarkably different in others. The upper [illegible]k and head is pale blue, behind each ear a spot of a darker shade of the same colour; the plumage otherwise similar to that of the kittiwake.

If it be not the *Larus Rissa*, it seems clearly undescribed: if it be, it at least presents a variety of habits before unascribed to that species.

In Zetland, a few of the young of the kittiwake remain during the winter, and they are sufficiently clearly marked throughout that season by the black bill, bars of the same colour across the wings, at the tips of the tail-feathers, and almost surrounding the neck; but from them, the bird I allude to differs remarkably, and it seems as different from the adult kittiwake as the *Larus canus* is. I had frequently observed these birds in Zetland towards the end of autumn, in small flocks, generally frequenting exposed bays; but taking it indolently for granted that they were merely the young of the kittiwake, I had not thought of observing them attentively.

Wishing, however, last autumn, to procure some specimens, and observing them unusually numerous, their peculiarities were more attended to. The plumage of all seemed perfectly and uniformly alike, and peculiarly full,—much more so, it occurred to me, than that of the kittiwake; quite like what I have remarked in the silvery gull. Its skin was very thick, and defended by a layer of fat, such as may be found in the same situation in the auks; and these are peculiarities I have usually observed in arctic birds. It was also in a much greater degree difficult to kill than the kittiwake. Great numbers were frequenting the coast, and were apparently devoid of fear. Multitudes were taken with a line and fish-bait. They seemed, on their first appearance, very much emaciated and fatigued, I presume from the effects of recent and protracted migration, but even then weighed fully as much as the kittiwake in its best plight. Its form seemed more elegant, erect, and elongated. Its mode of flight occurred to me to be different, not unlike that of the lapwing. Its sexual organs were very distinct, not at all obscure, like those of the young kittiwake, from which it was well distinguished, when they were seen contrasted with each other. If these, then, be the same, how happened they to present such differences at the same season? And how is it accounted for, that these birds should make their appearance on the coast, after the migration of the species had been so complete, as to leave only a few straggling young behind? And how, also, on this supposition, can it be imagined, that they should so suddenly and uniformly have changed their plumage so very far from the few that had remained behind, and which they must have perfectly resembled not two months before, and now differing not only so much in plumage, but also in habits, while not a single adult bird was found accompanying them, though their departure must have taken place together? But perhaps they were not the young of that year. How, then, is this difficulty avoided, that, when the kittiwakes arrive in Zetland in spring, all present, without one exception, the adult plumage? Yet the young are not then so old by several months as on this supposition this bird must have been.

This variety was to be found only for about a month in the neighbourhood, where it was first so numerous, and soon almost entirely disappeared, while some of the young kittiwakes still remained.

The supposition that it is merely the young kittiwake hatched in higher latitudes than Zetland, and therefore probably more advanced in plumage than those of the same age produced in that country, does not account for the total absence in the present instance of the parent birds, independently of other objections; nor am I quite certain that the kittiwake does breed in Greenland, as has been alleged. It may perhaps appear that it has been confounded with this bird, and that this is the only Greenland kittiwake. Possibly this gull may have the same relation to the *Larus canus*, that the *Larus Islandicus* (described in the 4th volume of the Wernerian Transactions), has to the herring gull; for it will not be, I suspect, disputed, that the Iceland gull is a distinct species, whatever ornithologist may claim the priority of its full and accurate description.

Unwilling as I am to multiply species, anxious rather to simplify and reduce them, I yet confess my inability at present to account for the peculiarities in the habits of this bird, but on the presumption of [illegible] a separate species, a species distinct at least from any d[illegible] gull.

It is termed in Zetland *Craa maa*, or the Crow gull, though I think they apply there the same name to the young kittiwakes after they have begun to fly, much in the same way as they denominate the young of two or three other species of gull *Scoree*. If it then be an undescribed species, we might have a *Larus corvus* added to the list.

As, however, I have only had my attention directed to it since last autumn, what I have here stated regarding it must be considered as coming from limited experience, and rather as hints for the remarks of others, than as ascertained definitively. I shall, however, continue my inquiries regarding this doubtful bird, and communicate their result to the Society.

3. *Colymbus Grylle, or Black Guillemot.*

Some obscurity still exists regarding the history of this species, which it is my present object to endeavour to remove.

One opinion maintains, that this species changes its black summer plumage to a grey mottled appearance in winter. Another considers this difference of appearance to depend on difference of species,—while a third refers it merely to the distinction of age; the black being the adult, the grey the young bird.

This latter opinion is that which I entertain, and I conceive it to be the only one capable of avoiding the inconsistencies that attach to the others.

The black guillemot has been stated to produce only one young during the year. This, however, is incorrect. It lays two eggs; and very seldom indeed is its nest found without two young ones. If this species do not migrate from Zetland during the winter, it ought then to occur there at that season in considerably greater numbers than in summer. This is, however, decidedly opposed to all my observations. The number, on the contrary, is certainly less.

Partial migration, then, does take place; and nothing appears more easy to suppose, than that the old migrate, while the greater part of the young remain behind.

The departure of the black individuals occurs soon after the young have quitted the cliffs; and when migration might be expected to take place. This disappearance is general and sudden, for in autumn I have not observed any of this species in stages of plumage intermediate between the grey and black.

The reappearance of the black in spring is also sudden, and the number of this species at that season appears also greater than in winter, and it is in spring that the different steps of change, from the grey to the black, are well marked. All this quite accords with the opinion I have adopted, and I suspect with hardly any other.

If even any unequivocal instance could be produced of an adult guillemot assuming, during winter, the plumage of the young, it would not invalidate the general conclusion I have stated; for still the undeniable fact would remain, and be unaccounted for by the hypothesis to which I object, that many perfectly adult birds are met with during winter; and if exceptions are admissible to the one opinion, they are equally so to the other.

But the observation of the transition of the old to the plumage of the young could effectually be made, only when the bird was in a state of confinement, and in a few solitary instances; yet to this experiment a valid objection might be stated, that from domestication producing its well known effects in varying the plumage, and modifying the habits of animals, this experiment of itself might establish nothing general in respect to what regularly occurs in their wild and native state. I do not deny that such an experiment would furnish a strong confirmation of the opinion which I am opposing; but still the other difficulties would have to be surmounted.

We see the young passing by progressive steps to the plumage of the old, but we remark no reverse gradation. But, if the opinion I have endeavoured to refute be correct, the change in the latter case must be instantaneously and universally effected.

The plumage which, it is asserted, the parent bird presents in winter, is *precisely* that of its young; and why it should assume this rather than any other, is perhaps a little suspicious.

The climates where this supposed winter change is accomplished are not all Arctic ones; and the opinion which conjectures this change, states no peculiarity in this species that can account for this singular and radical deviation from the habits of other similar birds.

The individuals I have met with in Zetland during winter, especially in the beginning of that season, are less sensible to danger, and dive less readily on the fire. These facts, I remember, struck me before I had formed any idea regarding the migration or change of plumage of this species, and are characteristic of young birds.

Frequently black guillemots are found during every period of the winter; and that such appearances are rather exceptions to a general law of migration, apparently not very imperative, than to that of change of plumage, seems too obvious to be insisted on.

The general habit of this genus is to carry the young to sea some time before they are capable of using their wings for flying, at least to any considerable distance; and, hence from deriving their food chiefly from diving, and from the comparative absence or disuse of flying, we naturally might expect that the greater part of the young would be unfit for distant or protracted migration, at the period of the disappearance of the old birds.

The only objection that occurs to the opinion I have adopted seems easily removed. It is, that the young should so far differ from the adult birds in their habits of migration. But, whatever opinion be held as to its cause, the fact is certain, that the numbers found in Zetland in autumn are much less than those met with in that country during winter. And to account for this, the supposition, as the general principle, of some of the old and young promiscuously departing and remaining, is surely quite as inadequate as the other, and besides, requires the admission of what is chiefly disputed, that the plumage of the old is changed to that of the young. But there is nothing, *a priori*, to prevent the supposition, that the young of certain species of birds should inhabit, at certain seasons, countries different from those resorted to by the parent birds; and this presumption is amply confirmed by experience and analogy, and derives still more forcible confirmation as we descend in the zoological scale. Why, indeed, young birds should not differ in habits of migration, as they do so frequently in characters of plumage from the perfect species, it is not easy to believe; and here what seemed a plausible objection to the opinion, becomes another presumption of its accuracy.

The final causes of migration have been often too restricted, confined almost exclusively to incubation, food, and climate; but these causes hardly apply in the present case; for the individuals of this species that winter in Zetland are in as good plight, and possess as great an abundance of food, as at any other season. We must recollect that animals do not exist merely to preserve themselves, and to continue their species, but to minister to similar purposes in others, to become their food, or to diminish their number; and migration facilitates these objects. What, for instance, should have become of one of the chief sources of the wealth of the Icelander, or of the "light and life" of the Greenlander, if the eider-ducks, in the one case, were to remain all the year, where so many pass the winter; or, in the other, if the seals should think proper to prolong their spring or autumn *jaunts* to those mysterious and unknown regions, where the hungry and superstitious fancy of the Esquimaux, impatient for their return, delights to exhaust itself?

But, the young birds may not be able to obey this useful and powerful law of migration, from not possessing strength sufficient to encounter the same fatigues and hardships as the old; and hence are left behind till they have acquired the perfect powers of their kind.

Temporary and local circumstances may also give rise to habits of migration, as well as instincts, proceeding from causes of general and permanent operation, and these may be continued through many generations, when the accident or necessity which first produced them no longer exists. Illustrations of this remark are familiar to every practical naturalist.

The Grey Guillemots found in Zetland in winter, are therefore merely the young of the Black Guillemot remaining behind, while the parent birds migrate, and perhaps carry along with them a few of the more advanced and vigorous of their young.

Colymbus Minor or Lesser Guillemot.—*Alca Pica* or Black-billed Auk.

These I concur with that indefatigable and intelligent ornithologist Dr Latham, in considering as the immature birds of the foolish guillemot and razor-bill. My reasons for this opinion I shall reserve till the observations I propose making on these species during this summer in Zetland be completed.

Montagu's reasoning on the other side, is very full and ingenious, though certainly founded on too partial observation.

EDINBURGH,
21*st April* 1822.

ART. XVII.—*On a species of Earthy Matter spontaneously Combustible.* By JOHN MURRAY, Esq. F. L. S., M. W. S., &c. &c. Communicated by the Author.

AS you had the goodnes to insert in a former number of the Edinburgh Philosophical Journal, the results of a chemical investigation of the properties of a liquid matter which I collected in the crater of Vesuvius, I have taken the liberty of submitting a description of a peculiar species of earthy matter, dug up at *Ashleyhag*, in the parish of *Wirksworth*, *Derbyshire*, which is remarkable for its *spontaneous combustion*. I have not been able to submit it to a very minute examination, but intend to do this when at leisure, and I may then have it in my power to send you a more rigorous analysis. This remarkable earthy matter was discovered about fifteen years ago, about six feet below the surface soil, by labourers engaged in "soughing" some land, situated on a declivity nearly at the base and SW. side of Allpont, the greatest elevation in the south of Derbyshire. The following are the strata incumbent on this substance:

Surface soil, from 6 to 8 inches deep.	
White clay,	2 feet thick.
Blue clay,	3 feet thick.

Then proceeds,

The substance referred to, 3 feet thick.

Underneath this is a stratum of lapideous matter, called by the people *Toadstone*, (amygdaloid,) but which is a simple aggregated mass or *Breccia*, composed of fragmented pieces of a dark red sandstone, agglutinated by peroxide of iron. The water beneath these deposits is an ochrey sediment.

This peculiar matter was immediately pronounced to be a rich and valuable soil, and consequently a considerable quantity was removed and put up into a heap near the garden wall, for the purpose of employing it in horticulture. It had only lain in this form twelve or fourteen days, when it emitted *copious volumes of smoke*, accompanied with *a powerful sulphureous smell.* The farmers, in order to extinguish it, ordered *water* to be thrown into the mass. This, however, only increased the evil, and, at the imminent hazard of suffocation, it was necessarily removed to a distance.

A small quantity had been scattered on some meadow-land. The grass *immediately withered*, and several years elapsed before it recovered its wonted fertility.

The residue of *two cart loads* (after this spontaneous combustion), would not fill a *wheelbarrow.*

The external or physical characters of this substance would induce us to believe it to be a rich dark mould. It is mingled with fragments and fibres of decayed wood, and with glistening metallic particles. It has a considerable avidity for moisture, and soon becomes humid. Dissolved in distilled water it possesses a ferruginous and styptic taste.

When ignited in a platinum spoon with a spirit-lamp, it becomes light brown, with interspersed minute shining particles, apparently metallic. It glows in this flame like pyrophorus, giving off copious and sulphureous vapours. The sulphurous acid gas thus found was announced by its peculiar smell and dense smoke, when a stopper moistened with ammonia was brought near.

400 grains ignited in a platinum crucible left only 66.5 grains. The loss, therefore, of 333.5 grains was principally carbonaceous matter and sulphur.

400 grains acted o iatic acid left 178.25 grains. This loss of 221.75 grains antity soluble in that acid.

A portion was dis water, and the solution passed through filtering paper. This was examined by reagents.

Sulphuric acid did not darken the solution, consequently the absence of soluble *vegetable* matter was inferred.

Alcoholic solution of *iodine* produced a *muddy cinnamon* colour, and a shining glassy film pervaded its surface.

Muriate of ammonia exhibited no traces of *alumina.* A slight effervescence was produced with *water*, as well as with *diluted acids*, probably attributable to the *acidification of the sulphur.*

Phosphate of soda, &c. gave, by a cloudy opacity, indications of *magnesia.*

Oxalate of ammonia produced copious white clouds, and indicated *lime.*

Succinate of ammonia and *tincture of galls*, evinced the presence of *iron*, which was more distinctly and less equivocally indicated by the *prussiates of potassa and ammonia.*

Pure ammonia imparted a slight *blue* tinge, and a cylinder of *polished iron*, after some hours, exhibited a *coppery film*; the presence of *copper* was therefore presumed.

Sulphuret of ammonia changed the solution as *black* as ink.

Chromate of potassa produced a *yellow* precipitate, and *hydroiodate of potassa* yielded *yellowish-green flocculi*, which subsequently settled into a *lighter yellow* colour; *lead*, therefore, was clearly determined.

As *nitro-muriate of platinum* indicated *no change* whatever, it was evident that *potassa* was absent.

Solutions of silver evinced, by copious curdy precipitates, that *muriates* were present.

Muriate and *nitrate* of baryta, in like manner, proved that *sulphates abounded considerably.*

From these exhibitions, its probable constituents, *on the effusion of water*, are carbonaceous matter, *muriate of soda* and *magnesia*, and the *sulphates* of *lime*, *lead*, *copper*, and *iron.*

Its spontaneous ignition may be accounted for, by the united action of air and water on the sulphur, in contact with metalline and carbonaceous matter, analogous to the phenomena sometimes exhibited in the aluminous schistus at the Hurlet mine, near Paisley; or, that of a paste of sulphur and iron-filings, when moistened.

ART. XVIII.—*On the Methods of separating Lime from Magnesia*[*]. By C. DAUBENY, M. D. M. G. S.

IT has often struck me, that the discordant results which the experiments of different chemists so often exhibit, where the analysis of the same substance is attempted by different means, may in part be explained, from their overlooking the affinity which mutually subsists between two salifiable bases, or between the compounds which they respectively form with acids, when both are dissolved in a common menstruum.

I do not pretend, indeed, to bring forward as original, an observation too obvious in its nature not to have occurred to other chemists[†]; and I even recollect, that the late ingenious Dr Murray, whose death is so great a loss to science in general, and to this branch of it in particular, appeals to the above principle, in attempting to account for the manner in which substances that, under existing circumstances, are found to be sparingly soluble in water, might have been brought into combination with it, by the co-operating affinity of other bodies already contained in the general solvent[‡].

It nevertheless appears to me, that this circumstance has been too little attended to as affecting the operation of the general laws of affinity between bodies in opposite electrical states, although its importance will be obvious, from a very few considerations.

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[*] Read before the Wernerian Society, 18th May 1822.

[†] Dr Young, in his Numerical Table of Elective Attractions, published in the Philosophical Transactions for 1809, remarks, " It is not impossible that there may be cases in which the presence of a fourth substance, besides the two ingredients of the salt, and the medium in which they are dissolved, may influence the precise force of their mutual attraction, either by affecting the solubility of the salt, or by some other unknown means; but there is reason to think that such cases are rare." I am disposed to differ with that acute philosopher on the latter point; but am happy to adduce the authority of his name in favour of the possibility of such an accident.

[‡] See his " Compar the Huttonian and Wernerian Theories," which is perhaps the best et appeared to Mr Playfair's eloquent work.

Admitting such an affinity as we suppose to exist between two earths, one of which it is our intention to disengage from its present combination, through the agency of another acid, it is very possible that if the *divellent* and *quiescent* attractions are already nearly balanced, the intervention of the third substance may altogether prevent a decomposition from taking place, which, under ordinary circumstances, would have been effected.

Without putting so extreme a case, nothing is more likely than that the affinity between two bases, existing together in the same solution, should so modify the results obtained by the action of re-agents upon either of them, as to augment or diminish materially the quantity of precipitate thrown down.

That the latter case at least is not an hypothetical one, I shall have occasion to shew, in the course of the present paper, but there are abundance of facts already familiar to chemists, that can only be explained on this supposition, such, for example, as the increased solubility (or the reverse) of salts in water, containing some third substance in solution, and the resistance which metals oppose to the action of acids, when united with certain other bodies *.

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* My friend the Rev. J. Coneybeare, has communicated to me the following fact, which confirms the view I have taken of this subject.

If chromate of lead and carbonate of potassa are fused, a double decomposition is supposed to take place, chromate of potassa and carbonate of lead being the results, so that the former, from its solubility in water, might, according to this view of the case, be separated from the latter by repeated washing. Mr Coneybear, however, finds that the yellow solution contains an admixture of chromate of lead, which may be separated by diluted nitric acid. After the separation of this precipitate, (the further addition of nitric acid producing no longer any such effect,) the chromic acid or chromate of potash may be obtained by evaporation in the mode usually prescribed.

The fact does not appear to admit of a ready explanation, unless we suppose that the co-operating affinity of the chromate of potash for the chromate of lead, enabled the water to take up a portion of the latter salt, for which it has not unassisted, an affinity powerful enough to overcome the cohesive attraction existing between the particles of the substance itself.

The circumstance of a portion of chromate of potash remaining undecompounded, might be adduced in favour of Berthollet's views.

Those who consider that entire insolubility among salts, is a property nearly as uncommon as perfect infusibility among bodies in general, will consider this fact analogous to those already well ascertained, where water is enabled to take up a

Having now pointed out the general principles to which I allude, and referring to the facts that will be detailed in the course of this paper for a further illustration of it, I proceed to comment upon one of the oldest and most favourite methods of separating lime from magnesia, I mean, that of combining both with sulphuric acid, and availing ourselves of the different solubility of the two salts thus formed, as a means of separating them.

Of the formulæ that have been recommended for this purpose, the one mentioned by Mr Phillips in the sixth volume of the "Journal of Science," appears to me the most satisfactory, and to this I intend to confine myself, conceiving that any objections that may be raised against his plan, apply with equal force to the methods proposed by other chemists, so far as they are dependent on the same general principle.

Mr Phillips recommends, that the earth suspected to contain magnesia should be dissolved in muriatic acid, that the muriates should be decomposed by sulphate of ammonia, the ammoniacal salts and water driven off by exposing the whole, after evaporation, to a red heat, and the residuum, after this process, (considered as consisting merely of the sulphates of lime and magnesia,) treated with water already saturated with sulphate of lime, in order to dissolve the magnesian without taking up any of the calcareous sulphate.

By these means, it is presumed, that the difference in the weight of the dried salt before and after the solution of sulphate of lime has been added, will give the proportion of sulphate of magnesia present, and that we may safely infer that of the calcareous sulphate from the residuum after such addition.

Ingenious as this process is, I shall state some objections it seems liable to; the first knowledge of which I owe in a great measure to a paper published in the 12th volume of the *Annales de Chimie*, by M. Longchamp.

It is directed by Mr Phillips, that the solution should be exposed, after the conversion of the muriatic salts into sulphates, to a temperature capable of expelling the ammoniacal salts and the water present. M. Longchamp, however, shews, that the

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fresh portion of a partic h it had previously become saturated, in consequence of the adn edient.

operator, in applying heat, is placed in the dilemma of either not driving off the whole of the water contained in the sulphate of magnesia, and I may add, of not subliming all the muriate and sulphate of ammonia present, or of separating a part of the acid combined with the magnesia.

In the former case, he will of course overrate the quantity of magnesia present, by regarding the sulphate as dry, whereas it will contain, according to M. Longchamp, nearly $\frac{1}{8}$th of its water of crystallisation, and in the latter he is in danger of estimating it too low, not merely from considering the sulphate as though it were a *neutral* instead of a *subsalt*, but also from confounding a part of it with the sulphate of lime left behind, owing to the very sparing solubility of the magnesian salt, after exposure to a considerable heat.

In order to ascertain the correctness of this statement, I began by submitting a given quantity of crystallised Epsom salt to a temperature not exceeding 212°, and found that it was reduced from 20 grains to 14.44, and that the residuum dissolved readily in water.

I then exposed the same quantity of the salt to a strong red heat for about three hours, at the end of which time it weighed only 8.3 grains., and when treated with water, left an insoluble powder, amounting to 0.7. It was evident from this, that the salt had undergone some change from the heat which had been applied; and that this was in part at least connected with a loss of acid, I proved by comparing the quantity of precipitate that could be obtained, by adding muriate of barytes to the solution of the salt which had been submitted to a temperature of 212° only, and that which had been exposed to a red heat.

From the former I obtained no less than 20.15 grs. of sulphate of barytes, whilst the latter yielded only 15.7 grs. Great care was taken that both precipitates should be dried under precisely similar circumstances, so that the ratio which they bear to each other may be relied upon as correct; but it seems probable that neither of them was reduced to the extreme point of dryness, as 20.15 grs. of sulphate of barytes would indicate 6.8 gr. of sulphuric acid, whereas 20. gr. of crystallised sulphate of magnesia contain only 6.5, and ought therefore to form no more than 19.2 gr. of the barytic salt.

If, therefore, the real weight of the precipitate in the former instance be put at 19.2, it will stand in the latter at 14.97; for, as

20.15 : 19.2 :: 15.7 : 14.97.

Now, 14.97 of sulphate of barytes indicate about 5.1 of sulphuric acid, or 1.4 less than was present in the same quantity of salt, which had been exposed only to a heat of 212°.

It is worth remarking, that 1.4 gr. of sulphuric acid exceeds only by 0.05 the quantity that would be required to neutralise 0.7 of magnesia, the exact amount of the residuum which the water did not take up. This residuum I treated with muriatic acid, which dissolved the whole of it; and on adding muriate of barytes, I perceived a cloudiness, but so slight a one, that the quantity of sulphuric acid present was inappreciable from its minuteness.

Upon the whole, therefore, these results confirm each other as nearly as can be expected, and lead to the inference, that if sulphate of magnesia be exposed to the heat of boiling water only, little more than half of its water of crystallization will be expelled; and if, to insure the separation of the remainder, a more elevated temperature be made use of, the residuum is then resolved into two salts, the one probably a neutral sulphate, soluble in water, the other a subsulphate, retaining but a minute proportion of acid, and nearly insoluble.

This may perhaps be seen more clearly by the following comparative sketch of the results obtained:

20 grains of crystallised Sulphate of Magnesia, exposed to a temperature of 212°,

Lost,	-	5.56 gr.	10.3 gr. of Water of Crystallisation.		
Retained,	-	4.74			
———	-	9.70	Dry Salt.		
		20.00			

Ditto, exposed to a red heat,

Lost,	Acid,	1.4	11.7 gr.		
——	Water,	10.3			
Retained,	Acid,	5.1			
	Magnesia,	3.2	viz.	as a soluble sulphate,	2.5
				insoluble ditto,	0.7
		20.0			3.2

It was desirable, however, to ascertain whether the heat might not be so regulated, [illegible] to duration and intensity, as to expel the whole [illegible] thout reaching the point at which the acid woul[illegible]

For this purpose, as well as for the sake of establishing more fully the facts already laid down, I took 50 gr. of crystallised Epsom salt, and exposed it in a platina capsule to the flame of a gas-lamp *, increasing the heat at successive intervals, and noting down the loss of weight each time.

The moderate heat first applied, caused in half an hour a loss of 21.5 gr. or 43 *per cent.*; but a continuance of the same for two hours drove off only 0.3 gr. more. I therefore increased the heat to the highest point which the gas-burner could afford, and found, that, at the expiration of an hour, the salt had lost 2.1 gr. more, making the whole loss 23.9 gr. after which a continuance of the same heat appeared to cause no farther change.

Now, the quantity of water present in 50 gr. of crystallised sulphate of magnesia, amounts, according to the last experiment, (which agrees pretty exactly with other accounts), to 25.75 gr.; so that nearly 6 *per cent.* of water was retained, after four hours exposure to the highest temperature that could be obtained in this manner.

I then took the same quantity of the salt, and exposed it for a similar period of time in a platina crucible to a dull red heat, by which it was reduced from 50 to 23.7 gr. It had therefore sustained a loss of 26.3 gr. only 0.5 more than the quantity of water which the crystallised salt must have contained; so that the heat applied in this instance may be presumed to approach as nearly as possible to that degree which was calculated (if any) to expel the water, without driving off the acid.

Nevertheless, I found that the solubility of the salt was in this instance considerably affected; for 10 gr. of the residuum, heated in a tube with twelve times its weight of water, were only partially dissolved. So altered, indeed, in this and other of its properties, did the salt appear, that it is probable that more of the acid had been driven off than the difference between 26.3 gr.

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* The gas-lamp promises to be an instrument of great utility in chemical research, and will almost supersede the employment of oil-lamps, in situations where a supply of the former can be readily obtained. Its advantages are, the great range of temperature which it affords, from 90° to 100° Fahr. to a greater heat than can be obtained by any oil-lamp,—its steadiness, and continuance for any length of time,—its safety, and cleanliness.

the amount of the loss sustained, and 25.75, the proportion of water originally present; so that it seems not unfair to conclude that the acid begins to be expelled before all the water is separated, and that in this instance a portion of the latter was still retained.

In order to learn how much further the weight of the salt might be affected by increase of temperature, I exposed the residuum for two hours to a stronger heat than had been before employed, and detected at the end a further loss of weight, amounting to 10 grains for the whole quantity.

Submitted for two hours longer to the same temperature, the weight of the salt was diminished by 3.3; but it appeared to have parted with nearly all the acid which could be separated by heat, as three hours continuance in the same temperature caused a further diminution of scarcely 0.7 gr.

The residuum, weighing just 10 grains, appeared to be wholly insoluble in water, but was entirely taken up by muriatic acid, emitting, however, during the time that the solution was going on, a smell of sulphuretted hydrogen, which seemed to indicate, that, contrary to the received opinion, sulphate of magnesia is convertible into a sulphuret by a continued heat.

Thus, it appears, that crystallised sulphate of magnesia loses at least 80 *per cent.* of its weight, when exposed for some time to a high temperature; but that it retains to the last a portion of its acid, was evident from the cloud caused in the muriatic solution, by adding a few drops of muriate of barytes, although, as 100 gr. of the crystallised sulphate contain about 16 of base, we cannot estimate the quantity of acid higher than at about $\frac{1}{7}$th of the whole. It would appear by Berzelius' statement, that magnesia is precipitated from its solutions in sulphuric and muriatic acid by ammonia, in the state of a *subsalt*, and not of a pure hydrate; but he rates the proportion of acid in these cases much lower, for in the subsulphate he found magnesia 67.5 *per cent.*, sulphuric acid 1.6, water 30.9.

The experiments just detailed, seem to establish the truth of the facts which I mentioned, as calculated to throw considerable doubt upon the ex [illegible] Mr Phillips' method; but I flatter myself that the [illegible] be obviated by the following simple addition to [illegible] has recommended. Having dissolved the lime and magnesia in muriatic acid, precipitate both by means of some alkali or alkaline carbonate, and ascertain their weight. Then redissolve them in an acid *, and follow the subsequent steps recommended in Mr Phillips' formula.

In this manner, provided only that after the ammoniacal salts have been driven off by heat, we treat the residuum with such a quantity of water saturated with sulphate of lime, as will carry off the sulphat of magnesia, we may calculate, from the amount of sulphate of lime left, how much of the precipitate collected in the former instance consisted of lime, and therefore what portion of it was magnesia.

Indeed, if this modification of the process be adopted, it will be scarcely necessary, unless where great exactness is required, to reduce the sulphate to dryness at all, as the supernatant liquor, after the addition of the sulphate of ammonia, can only contain 1 gr. of sulphate of lime in the ounce, and, if in sufficient quantity, must carry off with it the whole of the magnesian and ammoniacal salts. We shall thus escape the risk of applying such a heat as would drive off a portion of the sulphuric acid, and thereby diminish the solubility of the magnesian salt.

It has been suggested, however, by Dr Thomson †, that the presence of magnesia may tend to increase the solvent power of water with respect to the calcareous sulphate; and I felt the more desirous of determining this point, as such an idea coincided with the views which I have found reason to entertain, with regard to the influence of the affinity subsisting between two bases, in modifying the action of a solvent on one or both of them.

I therefore began by mixing together two solutions, (each of 2 oz.), the one containing about 70 grains of sulphate of magnesia in the ounce, and the other saturated, or nearly so, with sulphate of lime. The mixture was then poured into a long, narrow bottle, graduated so as to enable me to observe every successive diminution of volume, and exposed to a gentle but long continued heat in a sand-bath. The solution was then slowly

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* Distilled vinegar readily dissolves recently precipitated lime and magnesia, but does not act on alumina: it may therefore be conveniently employed in this stage of the process.

† Vide Annals of Philosophy, vol. xv.

concentrated by evaporation, until it measured only 1½ oz. when a slight appearance of a precipitate commenced. Being further diminished to 1¼ oz. the precipitate collected seemed to correspond pretty nearly to what might have been expected, had the solution consisted merely of sulphate of lime.

This experiment not being quite conclusive, I adopted the more direct method of ascertaining the quantity of sulphate of lime which would be dissolved by equal measures of pure water, and of a solution of the magnesian sulphate. I therefore took two portions of dry sulphate of lime, each weighing 5 grains, and poured upon the one 3½ oz. of pure water, and on the other the same quantity of a weak solution of sulphate of magnesia in the same fluid. Both liquors being after some time decanted off, the residuum from each was carefully washed with a quantity of water, as nearly as possible equal, and dried under precisely similar circumstances, when that which had been treated with distilled water only, was found to weigh 3.35 gr.; whilst the other was reduced by the saline solution to 3.16.

In order to confirm these results, I added oxalate of ammonia to the solution in which no sulphate of magnesia was present, and obtained from it a portion of oxalate of lime, which, when converted into a carbonate by heat, amounted to 1.42, equivalent to 1.92 of dry sulphate of lime. This, indeed, is 0.27 more than we should infer from the former experiment, owing, probably, to the oxalate of lime being but imperfectly converted into a carbonate. The two experiments, however, serve in the main to confirm each other.

The same process was then repeated with the calcareo-magnesian solution, but with this difference in the result, that the oxalate of ammonia produced at first no change whatever, and that afterwards, when the precipitate was collected, and had been exposed to a similar degree of heat, the amount of the sulphate of lime dissolved, appeared to have been only 1.225, less by 0.7 gr. than the quantity collected in the last experiment, and by 0.5 gr. than what would have been inferred from the loss sustained by the sulphate of lime, on which it was poured. I should therefor [illegible] ected some inaccuracy, had it not been for som [illegible] ts hereafter to be mentioned, which are such a [illegible] r, that the predominance of sulphate of magnesia in the latter solution prevented the decomposition of the last portions of sulphate of lime.

At all events, the whole tenor of these experiments controverts the notion of sulphate of lime becoming more soluble in consequence of the presence of sulphate of magnesia, and therefore removes that objection to Mr Phillips' method.

II. I proceed now to consider another of the methods used for separating the two earths, namely, that in which we employ the joint agency of carbonate of ammonia * and phosphate of soda,—a process which has received a sort of authority among chemists, from the distinguished name of Dr Wollaston, who first recommended it to public notice. Had that philosopher brought it forward as a precise method of determining the proportion of magnesia in a mineral body conjointly with lime, in those cases in which it enters as a principal ingredient into the composition of the mass, it might have seemed almost a superfluous task to have undertaken to confirm his results, and a piece of presumption for me to controvert them. As, however, I am not aware that this method was proposed by him, except as a very delicate test to indicate the presence of minute quantities of magnesia, I thought it not unnecessary to inquire, whether it could also be employed with ease and certainty, for determining the precise proportions which this earth might bear to lime, present with it in the same solution.

It is evident, that, for the latter purpose, it is as necessary that the carbonate of ammonia should be a complete precipitant of lime, as that the phosphate of soda should be so of magnesia, although, for the former intent, it may be sufficient, if we can depend merely upon the phosphate of soda. Now, with respect to the carbonate of ammonia, I have found, in all my experiments where this substance was employed, that a portion of lime remained unprecipitated, whether that earth was pre-

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* To avoid misapprehension, it may be as well to mention, that in this paper I have called by the name of Subcarbonate of Ammonia the salt sublimed by heating together muriate of ammonia and carbonate of lime; and by the term of *Carbonate*, the same salt after it has lost nearly all its pungency, by exposure to the air from twelve to twenty-four hours, in which time it seems to part with nearly half its weight. Neither salt, perhaps, is strictly speaking a definite compound.

sent alone, or in combination with magnesia; and this result was borne out, not only by comparing the quantities dissolved with those recovered, but also by the action of other tests, which evinced the presence of lime, after the carbonate of ammonia had ceased to produce any effect.

Thus, in one experiment, 10 gr. of pure lime just calcined, being treated with muriatic acid, which dissolved it without effervescence, and precipitated by carbonate of ammonia, yielded only 15.7 gr. of carbonate of lime, or 8.75 of lime. On adding oxalate of ammonia, a further precipitation took place.

In another experiment, 20 gr. of carbonate of lime, treated in the same manner, gave a precipitate which amounted to 17.58 gr. In this instance, by heating the residuum, so as to drive off the water, ammoniacal salts, and, lastly, the carbonic acid, I obtained a residuum, which answered in all its properties to pure lime, weighing about 0.8 gr. which indicates 1.42 of the carbonate.

We have thus accounted for $\frac{19}{20}$ths of the original quantity, the remaining $\frac{1}{20}$th probably consisting of water, combined with the carbonate of lime.

In order to assure myself more fully of the correctness of the former experiments, I took 10 gr. of carbonate of lime, precipitated by carbonate of ammonia from muriate of lime, which had itself been procured from a portion of the earth rendered as pure as possible. In this instance, there was a nearer approximation to the real quantity present than before, for the precipitate amounted to 9.35 gr. owing, I believe, to the solution having been set aside, after the ammoniacal salt had been added, for twenty-four hours, in order to insure the full operation of the re-agent.

It might be troublesome if I were to enter into the details of other experiments which I undertook, always with similar results. I shall therefore merely subjoin a list of the proportions *per cent.* of the lime originally employed, which I recovered in those instances where I could best depend on the result:

1. From 10 grains of recently burned Lime,	88
2. —— 20 —— Carbonate of Lime,	85
3. —— 20 —— C[illegible] Lime, with 20 Carbonate of Magn[illegible]	82.5
4. —— 10 —— C[illegible]cipitated, from the Muriate w[illegible] of Magnesia,	94.6

The imperfect manner in which lime is precipitated from its solutions by carbonate of ammonia, even where it exists alone in the solution, may be attributed partly to the formation of a supercarbonate of lime, which is to a certain extent soluble in water, and partly to the presence of ammoniacal salts, some of which appear to take up a portion of lime *; in addition to which, we may probably, in those cases where a magnesian salt was present, attribute somewhat to the affinity existing between the latter and the calcareous sulphate.

The first of the causes assigned, is favoured by the difference of effect produced upon solutions of sulphate of lime by the bicarbonate and subcarbonate of ammonia. If the former be added, the transparency of the solution is not immediately affected, but the latter renders it instantly turbid. The second I infer from the fact, that a solution of muriate of ammonia, after having been digested on carbonate of lime, is rendered much more turbid by oxalate of ammonia than pure water, which had been suffered to remain for an equal length of time in contact with the same substance, and afterwards filtered. In both instances the presence of lime was manifest; but in the case of the solution of muriate of ammonia, it was evinced by a precipitate, whilst in that of the pure water it was indicated by only a slight cloudiness.

That the solubility of carbonate of lime is also influenced by the presence of magnesia, I infer, (independently of the more direct evidence to that effect, which will afterwards be adduced), from the simple fact that solutions of sulphate of lime, to which carbonate of ammonia has been added, do not manifest any cloudiness, even after the interval of an hour, if sulphate of magnesia be present, although solutions similar in all respects but in this become turbid in a much shorter interval, after the application of the same re-agent.

Owing, however, to the slowness with which carbonate of ammonia in all cases operates upon weak solutions containing lime, the influence which the presence of magnesia exerts upon the effect, may be better seen when the subcarbonate is employ-

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* I find that Mr Phillips has anticipated me in this remark, although, from having seen originally only some extracts from his paper in the *Annals of Philosophy*, I was not aware of it at the time.

ed. In the latter case, I find that the magnesian salt, in certain proportions, almost entirely prevents the precipitation of lime from a solution of its sulphate, and in all cases appears to suspend for a time the operation of this re-agent. This will best be seen by the following Table, where the proportions in which the two salts were mixed, and the effect produced by the subcarbonate of ammonia are briefly stated :

Subcarbonate of ammonia, in a nearly saturated solution of

1. Sulphate of Lime alone, -	caused an immediate dense cloud.
2. Sulphate of Lime, containing 1 gr. of Sulphate of Magnesia in 1 oz.	caused no immediate effect, but in a few minutes a cloud as dense as in No. 1.
3. Ditto, with 2 gr. of Sulphate of Magnesia in 1 oz.	caused at first no effect, but in about ¼ of an hour the solution became as turbid as No. 1. & 2.
4. Ditto, with 5 gr. of Sulphate of Magnesia in 1 oz.	caused at first no effect, but in rather more than an hour a slight cloudiness appeared.
5. Ditto, with 10 gr. of Sulphate of Magnesia in the 1 oz.	caused no effect at all till the next day, and then much slighter than in the rest.

I afterwards tried solutions containing a larger proportion of sulphate of magnesia, and, as might be expected, with results similar to those obtained in the two last experiments.

The same influence appears to be exerted on a solution of muriate of lime, by the presence of a predominating portion of muriate of magnesia; for the precipitate caused by the addition of subcarbonate of ammonia, is, in this case, partially or completely redissolved ; whereas, in the other, it remains permanent.

But not only does carbonate of ammonia appear, from the foregoing statement, to be at all times an imperfect precipitate of lime, and more especially so when magnesia is present, but it seems also to throw down a portion of the latter earth, even when care is taken not to decompose the triple carbonate of ammonia and magnesia, by employing heat.

This I infer from the cloudiness produced, in more than one instance, when I redissolved the precipitate thrown down by carbonate of ammonia in muriatic acid, decomposed the muriate of lime in the same manner as before, and added phosphate of soda to the filtered liquor.

It may strike one at first as singular, that carbonate of ammonia should not throw down, on its second application, the same quantity of magnesia as it did on the first; but a little consideration will explain to us the cause of this occurrence.

When the ammoniacal salt is added to a mixed solution of the two earths, it would seem that the magnesia is divided between the ammonia and the lime, somewhat in proportion to the degree of its affinity to the two, the larger portion being held in solution with the former, in the state of a triple salt, the smaller carried down with the latter as a simple carbonate.

When, however, the precipitate is redissolved in an acid *, and submitted a second time to the action of the same re-agent, it is evident that only a small proportion of the magnesia existing in the compound will be thrown down, as the carbonate of ammonia will exert the same affinity as before, and consequently form, with the larger portion, a triple salt soluble in water.

From the facts above stated, it seems to follow, that carbonate of ammonia is not to be depended on as a complete precipitate of lime. Let us now go on to consider, whether phosphate of soda can be employed to determine the proportion of magnesia.

I dissolved 1 gr. of crystallised sulphate of magnesia in 8000 gr. of water, and found that the addition of carbonate of ammonia and phosphate of soda produced a very sensible precipitate, in a portion of the liquor set aside in a watch-glass. Indeed, this salt, even when diluted with 12,000 gr. of water, was detected in this manner by a slight cloudiness. When a weak solution of sulphate of lime was substituted for pure water, the test proved less delicate; but even then it detected 2 gr. of sulphate of magnesia in a pint of the fluid.

Now, crystallised sulphate of magnesia, according to the latest experiments †, (which I am happy to find confirm those I have myself had occasion to make), consist of about

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* It would seem to follow in theory, that, by adding a larger quantity of the carbonate of ammonia, the whole of the magnesia might be retained in solution in the first instance. But it must be recollected, that, by increasing the quantity of the re-agent, more of the lime would probably be held dissolved.

† M. Longchamp, in his paper, states the proportions of water lower, and the base higher, than this; but Gay Lussac has since contradicted him, and given an estimate that agrees very nearly with that above.

Acid, - -	32
Base, - -	16
Water, - -	52
	100

Consequently, in the former instance, 16 parts of magnesia were contained in 800,000 parts of water, or 1 of the earth in 50,000 of the fluid; whilst in the latter, the respective proportions were as 1 to 24,000.

The same insensibility to the presence of magnesia, I find to be possessed by the phosphate of ammonia, which Dr Murray has proposed to substitute for phosphate of soda *; but I am not aware that this test, which is somewhat less easily procured, possesses any positive advantages over the latter, except where we wish to analyse the residual liquor after the separation of the magnesia, as in the examination of a mineral water.

In such cases, it is better no doubt to employ a salt having a base of ammonia, which, in all its combinations, admits of being readily disengaged, than to make use of a fixed salt, where the alkali might by its presence occasion some perplexity in our subsequent operations.

In order to ascertain the comparative accuracy of these two tests, I made use of given quantities of sulphate of magnesia, a salt, the composition of which, in its crystallised state, appears to be pretty accurately determined.

From 50 gr. of this substance dissolved in water, I obtained, by means of phosphate of soda, a triple salt, which, when dried under an exhausted receiver, in connection with recently fused muriate of lime, amounted to 45.8 gr., and was reduced by a red heat to 20.3; whilst, from the same quantity of sulphate of magnesia, treated with phosphate of ammonia, I obtained 44.5 gr. of triple phosphate, which, after exposure to the same temperature with the former precipitate, weighed 19.7.

But as it is difficult, by the method employed above, to deprive the ammoniacö-magnesian phosphate of the whole of its water †, and as a very inconsiderable heat decomposes

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* See Dr Murray's "General Formula for the Analysis of Mineral Waters," in the *Edinburgh Philosophical Transactions.*

† Unless this be admitted, we must suppose a larger quantity of ammonia to enter into the composition of the triple phosphate than Dr Marcet imagines, ac-

it *, I prefer calculating the quantity of magnesia from the residuum, after exposure to a red heat, taking it in both instances at 40 per cent. of the precipitate.

The analysis, therefore, by phosphate of soda, indicates 20.3 gr. of phosphate of magnesia, equivalent to 8.1 of the pure earth; that by phosphate of ammonia 19.7 gr., or 7.88 of base.

Now, we have stated, on the authority of the latest experiments, that 50 gr. of crystallised sulphate of magnesia contain 8.0 of base, which is nearly the mean between the two analyses.

To satisfy myself still further that the whole of the magnesia is thrown down by this re-agent, I observed whether potash, which (as I shall afterwards mention) is one of the most delicate tests of magnesia, produced any effect on a magnesian solution, after the alkaline phosphate had ceased to act; but although this re-agent was added in considerable quantity to water that had contained solutions both of sulphate and muriate of magnesia, the liquor remained, in either case, just as transparent as before.

(To be concluded in next Number.)

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cording to the calculation made by him of the proportion between the amount of the base present in the precipitate, before and after exposure to a red heat. For, as he estimates the former at 19, and the latter at 40 per cent. of the whole, the precipitate by phosphate of soda ought, in that case, to have been reduced from 45.8 gr. only to 21.75; and the one obtained by phosphate of ammonia from 44.5 to 21.14. For, as 40 : 19 : : 45.8 : 21.75; and as 45.8 : 21.75 : : 44.5 : 21.14. Whereas the residuum in the former was less by 1.45, and in the latter by 1.44. The circumstance that the two precipitates were reduced by a red heat, in exactly the same ratio, (corresponding to the $\frac{1}{100}$th of a grain), whilst it gives me some confidence in the accuracy of the results themselves, seems rather to favour the idea that the quantity of ammonia present exceeds Dr Marcet's statement, for it is not likely that the precipitates, after remaining under an exhausted receiver, should have both retained precisely the same proportion of water. I am unwilling, however, to call in question the experiments of so accurate a chemist, without having more solid grounds to go upon.

* I have found that the triple phosphate begins to be decomposed at a temperature very little exceeding 100° of Fahrenheit.

ART. XIX.—*Researches on Hydrocyanic Acid and Opium, with reference to their Counter-Poisons.* By JOHN MURRAY, Esq. F. L. S. M. W. S. &c. Communicated by the Author.

IN June 1815, a paper of mine was read to the Linnean Society, developing a simple and apparently decisive method of ascertaining the sedative virtues of vegetable juices and their counter-agents.

The sciatic nerves of the prepared frog were taken up by a silver probe, and moistened with the tincture, and the result indicated the sedative power or its obverse; the degree was determined by the specific gravity of the solution employed, and the power measured by the duration of the period required to produce its maximum effect.

It would be superfluous now to describe what has already been amply detailed. It was clearly proved from the result, that a suspension of the voltaic excitement, more or less decided, was the consequence of certain vegetable juices, and that in such as were operative in this manner, acetic acid was found to be a counter-agent.

It may be worthy of remark in this place, that discoveries have since manifested new alkaline bases, characterised by specific characters in such as having produced a sedative effect, were neutralised by acetic acid, as *morphia, atropia,* &c.

The following paper is intended simply to detail the results of some experiments, instituted with reference to the discovery of counter-poisons to their agency on the system. Facts are soon detailed; and it is not necessary that they be amplified or extended by unnecessary details. The truths gleaned from actual experiment are immutable, while the consequences which may be deduced in support of a theory, may soon be overlooked in the progression of intelligence.

I had always found, that the violent headach which sometimes occurred in preparing hydrocyanic or prussic acid, was relieved and removed by *ammonia*, which induced me to think that the antidote to that acid [illegible]t and formidable poison, might be found in ammoni[illegible]

A small portion of hydrocyanic acid was given to a healthy young rabbit, which proved fatal in ten minutes. Soon after its administration, the head declined on one side, violent spasm supervened, while the eye lost its lustre, and the animal died in dreadful convulsions.

On dissection after death, the lobes of the lungs appeared paler than usual, coagulable lymph was found lining the trachea, as in Cynanche Trachealis, and the stomach was found inflamed near the pylorus. The brain was not examined.

The muscular fibre was still excitable by voltaic agency, but the excitability soon declined.

A drop or two of hydrocyanic acid on the head of a frog soon proved fatal. The colour promptly changed to an unwonted paleness.

The sciatic nerves of the prepared limbs were moistened with hydrocyanic acid, but no suspension of the voltaic excitement supervened. It was accompanied by a tremulous movement of the muscular fibre, connected with the lines of the nerves; and this spontaneous irritability seemed increased by the application of alcoholic solution of iodine.

It is a singular fact, that not unfrequently an alcoholic solution of iodine, dropped on the muscular fibre of a frog, excited phenomena similar to the action of the voltaic apparatus. It seemed also to renew excitability when the susceptibility had declined or was lost.

When the symptoms were verging to a fatal issue in a frog, a drop or two of ammonia on the head effectually restored the animal.

A greater quantity of hydrocyanic acid was given to a young rabbit than proved fatal in the case detailed. Ammonia was occasionally applied to the mouth on a sponge. The animal exhibited no unhealthy symptom whatever.

A considerable quantity of the hydrocyanate of ammonia with excess of base, was administered to another rabbit, but without any deleterious effect.

Half a drachm of hydrocyanic acid was given to a healthy young rabbit. The effects were prompt. Respiration became laborious and difficult, with a grating in the throat,—the eye lost its brilliancy,—the head dropped,—it raised a sharp cry, and was convulsed. Strong ammonia was dropt into the animal's mouth, and it was repeatedly moistened with a sponge dipped into ammonia. It almost instantly revived, and even *licked repeatedly the finger* which sometimes applied the ammonia, apparently quite sensible of the instant and continued relief it afforded. The animal effectually recovered. Its lips were excoriated by the ammonia.

Conscious of the complete antidote to this formidable poison found in ammonia, I took a quantity of hydrocyanic acid, sufficient to produce violent headach, stupefaction, &c., but diluted ammonia afforded me instant relief. I occasionally applied it to the olfactory organs, and bathed the forehead.

Since hydrocyanic acid has been introduced into our pharmacopœia, and employed in *phthisis pulmonalis*, and accidental poisoning may be anticipated, it is of much moment to know an effectual barrier to its virulence; and such is my complete conviction of the antidote, that I would feel no hesitation whatever in taking a quantity sufficient to *prove fatal*, provided there stood by a skilful hand to administer the remedy.

It is admitted that *morphia* is the active principle in opium. Morphia dissolved in alcohol, in which, however, it is sparingly soluble, produced, on the sciatic nerves of a prepared frog, effects analogous to those of the tincture of opium. Acetic acid restored the voltaic excitability.

The sciatic nerves were moistened with superacetate of morphia, but the excitement was the same as if none had been applied.

A frog's head and abdominal viscera were steeped in superacetate of morphia, but the voltaic action remained unchanged.

Half a drachm of superacetate of morphia was given to a young rabbit, but no apparent derangement of its healthy functions took place: it rather seemed to act as a stimulus to appetite.

These experiments pointed out *acetic acid* as the counter-poison to opium, and from its volatile properties, and other characters, in which it differs almost essentially from *acetous acid*, having no affinity with it except in an acid character, and having much of the features of an ether, I am of opinion acetic acid may prove serviceable where acetous acid would not prove effectual.

Two and a half drachms of tincture of opium were given to a rabbit. In a short time the eye became more opake, the pupil dwindled to a mathematical point, and was insensible to the stimulus of light, the head fell to the floor, and the breathing was difficult and loud, and there supervened a fatal prostration of strength. *Acetic acid* was then administered through a quill, and applied to the mouth on a sponge repeatedly. The head was also bathed with acetic acid, and it was also applied to the extremities, and in the direction of the spine. The whole quantity of the acetic acid used was about a fluid ounce. The animal was also frequently roused, and finally kept warm. The animal effectually recovered.

These experiments were repeated with uniform success on other rabbits. Several days have elapsed, and they continue in the most healthy condition.

I much regret that these experiments have been so painful to me, as to cause for some time an interruption of my researches on *Hyoscyamus niger*, *Atropa Belladonna*, *Cicuta virosa*, and other vegetable poisons; and nothing but the high importance which might attach to the discovery of an antidote to their fatality, could have induced me to commence the inquiry.

I have no hesitation to pronounce, with positive certainty, that in ammonia will be found a complete antidote to hydrocyanic acid, and in acetic acid an effectual counter-poison to opium.

The agency of voltaic excitement holds out a method to discover the comparative sedative or narcotic properties of vegetable juices, as well as their counter-agents. It unfolds also those that are stimulating and those that are not, with their relative correctives. By this means, we are prepared by well grounded anticipation for the successful application of an antidote.

ART. XX.—*Report on the Cultivation of Spices at Bencoolen, one of the British Settlements in the Island of Sumatra; addressed to W. R. Jennings, Esq., Secretary to Government, Fort Marlborough.* By J. LUMSDAINE, Esq. *

THE geographical position of the Island, its local adaptations, and the genuine influence of its sky and climate on the vegetable kingdom, but, above all, the similarity of the latter to that of the Moluccas, induced a belief that the spice-trees would thrive as prosperously in these districts as in their native clime. Accordingly, the Deputy-Governor and Council of Fort Marlborough, dispatched, in 1796, a small vessel to Amboyna, for the purpose of throwing in supplies into that garrison, and returning with spice-plants. Owing, however, to some untoward accident on the voyage, it was found necessary to bear away for Prince of Wales' Island, where the vessel was declared not seaworthy, and thus the object of the mission was frustrated. It was nevertheless reattempted and accomplished in 1798, by the ship Phœnix, which landed 864 nutmeg, and 66 clove-plants at Fort Marlborough, the whole of the former, and two-thirds of the latter being in a healthy and vigorous condition. These were distributed to such of the gentlemen of the settlement and natives as engaged to take care of them, and a few were sent to the out settlements, in order to ascertain the soil most favourable for their culture. A considerable share fell to the lot of Mr Edward Coles, by whom they were planted out at Parmattang Ballam, in virgin forest-land, where the most forward of the nutmeg trees blossomed, and perfected their fruit towards the close of the year 1803. The cloves pined and dropped off in rapid succession. Out of the whole number, four only arrived at maturity, one of which flowered in 1803, and the most vigorous of them did not survive the thirteenth year.

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* Abridged from a Memoir in the proceedings of the Agricultural Society of Sumatra, printed at Bencoolen in 1821.

Notwithstanding the indifferent success attending this first essay from the loss of numbers of the plants, the general result was satisfactory, and inspired a belief that these valuable exotics might, by perseverance and increased attention, become naturalised to the soil. An opportunity of putting this to the test of further experiment accordingly presented itself in 1803, in which year the late Mr William Roxburgh reached the settlement with a supply of upwards of 22,000 vigorous nutmeg-plants, and between 6000 and 7000 clove-plants from Amboyna, which were put under charge of the late Mr Charles Campbell, for general distribution; but the applications for plants were so urgent, that they greatly overbalanced the stock imported.

The settlement now assumed the resemblance of a busy agricultural community, and discussions on the properties of the soil became the daily topics of conversation. The soil throughout the plantations is generally red mould, with stony fragments or pebbles frequently intermixed with it: the surface of it in the forest, alluvial and low lands, being of a chocolate colour, varying in depth from 3 to 10 or 12 inches. After a long duration of dry weather, this mould is frequently found to be so stiff and unyielding, as to require a good deal of labour to effect a mechanical division of its particles, so as to render it fit for the purposes of agriculture. It is susceptible, however, of very great amelioration by means of tillage and appropriate composts; and, indeed, the soil of Ceylon, in which the cinnamon tree thrives so luxuriantly, is of the same description. The low lands and swamps are highly productive, in proof of which it is sufficient to mention, that those of Benteerin were at one time under a course of culture for twenty years without a fallow. A marly-looking soapy clay is met with at the distance of 10 or 12 feet from the surface, which, at a still greater depth, is considerably indurated by the action of the water that percolates through the ground in all directions, and in combination with sand, forms the substance called Napal.

The mode of culture adopted in the different plantations is nearly the same. The beds of the trees are kept free from grass and noxious weeds by the hoe, and the plough is occasionally run along the interjacent spaces for the purpose of eradicating the Lallang (*Andropogon caricosum*), which proves greatly obstructive to the operations of agriculture. The trees are generally manured with cow-dung and burnt earth once a-year, in the rainy season; but the preparation of suitable composts, and their mode of application, are but imperfectly understood. The pruning-knife is too sparingly used; very few of the planters lop off the lower verticels of the nutmeg-trees, or thin them of the unproductive and straggling branches.

The site of a plantation is an object of primary importance, and doubtless the alluvial grounds are entitled to preference, from the acknowledged fertility of their soil, and its appropriate organisation and capability of retaining moisture, independently of the advantage of water-carriage. Several of the nutmeg-trees of the importation of 1798 at Moco Moco, are placed in soil of this description, and although never manured, are in the highest state of luxuriance, and bear abundantly. Next to the alluvial deposit, virgin forest-lands claim pre-eminence, their surface being covered with a dark coloured mould, formed by the slow decay of falling leaves and mouldering trunks of trees; and next to these are to be ranked the open plains. Declivities are objectionable, from the risk of the precipitation of the mould and manure into the subjacent ravines, by the heavy torrents of rain that occasionally deluge the country. Above all, the plantations must be protected from the southerly and northerly winds, by a skirting of lofty trees; and, if nature has not already made this provision, no time should be lost in belting the grounds with a double row of the Cassuarina littorea and Cerbera Manghas, which are well adapted for this purpose. If the plantation is extensive, subsidiary rows of these trees may be planted at convenient distances; but no large trees whatever should be suffered to grow among the spice-trees, as they exclude the vivifying rays of the sun, and arrest the descent of the salutary night-dews, both of which are essential to the quality and quantity of the produce, besides that they rob the soil of its fecundity, and intermingle their roots with those of the spice-trees. Extensive tracts of land are to be met with in the interior of the country, well adapted for the cultivation of the nutmeg and clove, and to these undoubted preference is due.

In originating a nutmeg plantation, the first care of the cultivator is to select ripe nuts, and to set them at the distance of a foot apart in a rich soil, merely covering them very lightly with mould. They are to be protected from the heat of the sun, occasionally weeded and watered in dry weather every other day. The seedlings may be expected to appear in from thirty to sixty days, and when four feet high, the healthiest and most luxuriant, having three or four verticels, are to be removed in the commencement of the rains to the plantation, previously cleared of trees and underwood, by burning and grubbing up their roots, and placed in holes dug for their reception at the distance of thirty feet from each other, screening them from the heat of the sun and violence of the winds. It is a matter of essential importance, that the ground be well opened, and its cohesion broken, in order to admit of the free expansion of the roots of the tender plants, and that it be intimately mixed with burnt earth and cow-manure, in the proportion of two-thirds of the former to one-third of the latter. The plants are to be set in rows, as well for the sake of regularity as for the more convenient traversing of the plough, which is now to be employed in clearing the intermediate spaces of lallang and other noxious grasses, carefully avoiding to trespass on the beds of the trees. They must be watered every other day in sultry weather, manured annually during the rains with four garden-baskets of the above mentioned compost to each tree, and protected from the sun till they attain the age of five years. They will then be sufficiently hardy to bear the sun, and from that age until their fifteenth year, the compost should consist of equal parts of cow-dung and burnt earth, and from eight to twelve baskets of this will be required for each bearing tree, a lesser proportion being distributed to the males. From the power of habit, the trees will, after the fifteenth year, require a more stimulating nutriment; the dung ought not, therefore, to be more than two or three months old, and the mixture should consist of two parts of it to one of burnt earth, of which the suitable proportion will be from twelve to sixteen baskets to each tree biennially.

In all plantations, whether situated in forest lands or in the plains, the necessity of manuring at stated intervals has been found indispensable, and is indeed identified with their prosperity. The proper mode of applying it is in a circular furrow, in immediate contact with the extremities of the fibrous roots, which may be called the absorbents of the plant. In a scarcity of dung, recourse may be had to the dregs remaining after the preparation of the oil from the fruit of the Arachis Hypogæa in mixture with burnt earth, which is a very stimulating manure; or composts may be formed by the decomposition of leaves or vegetable matter of any description. Sea-weeds and many other articles may also be resorted to, which will readily occur to the intelligent agriculturist.

During the progressive growth of the plantation, the beds of the trees are to be regularly weeded, and the roots kept properly covered with the mould, for these have a constant tendency to seek the surface; the growth of the lateral branches alone is to be encouraged, and all suckers or dead and unproductive branches are to be removed by the pruning-knife, so as to thin the trees considerably, and to admit of the descent of the night-dews, which are greatly contributive to their well-being, especially during the dry and sultry weather; creepers are to be dislodged, and the lower verticels lopped off, with the view of establishing an unimpeded circulation of air. The conclusion of the great annual harvest is the fittest time for pruning the trees.

The nutmeg tree is monœcious as well as diœcious, but no means of discovering the sexes before the period of inflorescence are as yet known. The relative proportion of male and female trees is also undefined, and is indeed the effect of chance; the number of productive trees may, however, in a rough way, be estimated at about two-thirds of the whole cultivation. Whatever may be the intention of nature in producing so great a proportion of male trees, and however necessary this may be to the continuation of the species in a wild state, it does not appear, that, when reduced to a state of cultivation, the fertile plants are in any way benefited by it, as the monœcious plants have in themselves the means of perfecting their fruit. The number of male trees, therefore, necessary to be retained, for the impregnation of the female ones, will depend entirely upon that of the monœcious kind; and all above this number are to be considered as superfluous, and should be cut down, that others may be planted in their stead.

Upon an average, the nutmeg-tree produces fruit at the age of seven years, and increases in produce till the fifteenth year, when its productiveness is at the highest. It is said to continue prolific in the Moluccas for seventy or eighty years, but our experience carries us no farther than twenty-two years and a half, all the trees of which age that have been properly managed, are still in the highest degree of vigour and fecundity; and for this reason no term for planting a succession of trees can yet be fixed upon. Seven months in general elapse between the first appearance of the blossom and ripening of the fruit, and the produce of one be [illegible] with another under good cultivation, may, in the fi [illegible] the plantation, be calculated at five pounds of n [illegible] ound and a quarter of mace.

I have observed, however, that some trees produce every year a great quantity of fruit, while others constantly give very little. It bears all the year round, but more plentifully in some months than in others. The great harvest may generally be looked for in the months of September, October, November and December, and a small one in April, May and June. Like other fruit-trees on this portion of Sumatra, I have remarked that it yields most abundantly every second year. The fruit having ripened, the outer integument bursts spontaneously, and is gathered by means of a hook attached to a long stick, and the mace being cautiously stripped off, and flattened by the hands in single layers, is placed on mats for three or four days in the sun to dry. In damp and rainy weather, the mace should be dried by the heat of a charcoal fire carefully conducted, so as not to smoke it or blacken its surface.

The nuts liberated from their macy envelope, are transported to the drying-house, and deposited on an elevated stage of split neebongs, placed at a sufficient distance from each other to admit of the heat, from a smouldering fire beneath, without suffering even the smallest nuts to pass through. The heat should not exceed 140° of Fahrenheit, for a sudden inordinate degree of heat dries up the kernels of the nuts too rapidly, and its continued application produces fissures in them; or a fermentation is excited in them, which increases their volume so greatly as to fill up the whole cavity of the shell, and to prevent them from rattling, when put to this criterion of due preparation. The fire is lighted in the evening, and kept up for the whole of the night. The smoking-house is a brick building, of a suitable size, with a terraced roof, and the stage is placed at an elevation of ten feet from the ground, having three divisions in it for the produce of different months. The nuts must be turned every second or third day, that they may all partake equally of the heat; and such as have undergone the smoking process for the period of two complete months, and rattle freely in the shell, are to be cracked with wooden mallets, the worm-eaten and shrivelled ones thrown out, and the good ones rubbed over simply with recently prepared well sifted dry lime. They are now to be regarbled, and finally packed for transportation in light casks, the insides of which have been smoked, cleaned, and covered with a coating of fresh water and lime. If packed in chests, the seams must be dammered to prevent the admission of air or water. There is no necessity for sorting them, as, previously to their sale, they are classed into sizes in the Company's warehouse in London.

The mode generally practised in preparing nutmegs for the market, is to dip them in a mixture of salt-water and lime, and to spread them out on mats for four or five days in the shade to dry. I am, however, convinced, from much experience, that this is a pernicious practice, not only from the quantity of moisture imbibed in this process, encouraging the breeding of insects, and rendering the nuts liable to early decay, but from the heating quality of the mixture producing fissures and occasioning a great loss in the out-turn; whereas, by liming them simply in the dry way, as I have recommended, the loss ought not to exceed 8 per cent. In the shell they will keep for a great length of time: I myself have kept them in this state for a great length of years, and when cracked they were found perfectly sound. From the report of the London brokers, however, they will not answer in this way in Europe, on account of the heavy allowance for shells, which is one-third of the weight; but the Chinese merchants are in the daily habit of exporting them to Pinang and China, where they are in request.

Although the clove-tree attains great perfection in the red mould of these districts, it is more partial to a less tenacious soil. Its cultivation has been established for many years in the West Indies and at Bourbon, and is of secondary importance only. The mother cloves are planted in rich mould, at the distance of twelve inches from each other, screened from the sun and duly watered. They germinate within five weeks, and when four feet high, are to be transplanted at intervals of thirty feet, with a small admixture of sand with the red mould, so as to reduce its tenacity, and are to be cultivated in the same mode as the nutmegs, only that when full grown, they require less manure in the proportion of one-third. They yield generally at the age of six years, and at that of twelve are in their highest state of bearing, when the average produce may be estimated at six or seven pounds of marketable fruit each tree during the harvest, which takes place in the rainy months, but with us they have hitherto borne two crops in three years only. The fruit is terminal, and when of a reddish hue, is plucked by the hand, so that the process of gathering it is tedious. It is then dried for several days on mats in the sun, until it breaks easily between the fingers, and assumes a dark brown colour. It loses about 60 per cent. in drying. When past its prime, the clove-tree has a ragged and uncombed appearance, and I am led to suppose that its existence is limited to twenty years, unless in very superior soil, in which it may drag out a protracted and unprofitable state of being to the period of perhaps twenty-four years. Hence it becomes necessary to plant a succession of seedlings, when the old trees have attained eight years of age, and this octennial succession must be steadily kept in view.

With reference to the number of labourers, cattle, and ploughs necessary for a plantation of 1000 nutmeg or clove-trees, after the ground has been thoroughly cleared of underwood and stumps of trees, I consider that seven Chinese, or active Bengalee labourers, fifty head of cattle, and two ploughs, would be sufficient for all the purposes of the cultivation, with the exception of collecting the clove harvest, which being a very tedious process, would require an extra number of hands, and, indeed, the best plan would be to gather it in by contract.

I have very great satisfaction in affording my individual testimony to the energy and zeal which actuate the great body of the planters, and of the correspondent improvement of their respective plantations. Without mentioning the names of individuals who have been foremost in this race of emulation, suffice it to say, that the plantations generally exhibit tokens of progressive amelioration, and that such of the trees of the importation of 1798 as have been duly cultured, are in the highest degree of health, vigour, and productiveness.

It would be unreasonable to expect that such felicitous results could have been realised without proportionate sacrifices. In the first era of the speculation, the cultivators had to contend, on the one hand, with Nature, in exploring and eliciting the latent properties of a soil notable only for its supposed indomitable sterility; while, on the other hand, the problematical success of the undertaking and extent of capital requisite to conduct it to a prosperous issue, involved considerations of no trivial importance. It is to their industry, spirit, and perseverance, that we owe the naturalisation of these valuable exotics, the established reputation of their produce, both in Europe and India, the abolition of the monopoly and exclusive pretensions of the Dutch to this trade; and, finally, the assured possession to Great Britain of this promised scene of national and colonial wealth. I consider that I am within bounds, in estimating the total amount of European private capital sunk in this speculation at 436,000 dollars, and of native at 35,000 dollars, from the first commencement of the plantations until the trees respectively came into bearing, but of this a considerable portion has been redeemed in produce.

Abstract Statement of the Public and Private Spice Plantations at and in the Vicinity of Fort Marlborough, for 1819–20.

		Above 20 years of age.		Between 10 and 20 years of age.		Between 5 and 10 years of age.		Total under 5 years of age, including nursery plants, not bearing.	Total No. in cultivation.
		Total No.	Bearing.	Total No.	Bearing.	Total No.	Bearing.		
Nutmegs.	1819—20	130	113	19,045	12,382	39,024	13,554	43,712	101,911
	1818—19	152	135	16,938	10,360	43,418	13,728	48,921	109,429
	Increase,			2107	2022				
	Decrease,	22	22			4394	174	5209	7518
Cloves.	1819—20			2132	3132	9832	5243	12,728	24,692
	1818—19			2160	1653	9200	4837	14,912	26,372
	Increase,				479	632	400		
	Decrease,			28				2184	1680

Art. XXI.—*On a Remarkable Peculiarity in the Law of the extraordinary Refraction of differently-coloured Rays exhibited by certain Varieties of Apophyllite.* By J. F. W. Herschel, Esq. F. R. S. Lond. & Edin. &c. &c. &c. [*]

Doubly refracting crystals have hitherto been divided into two classes; the first, comprising those in which the deviation of the extraordinary ray may be regarded as arising from a repulsive force, emanating from one or more axes, as in carbonate of lime; and the other, from an attractive, as in zircon. Intermediate between these, and forming, as it were, the limit between both, are bodies devoid of the property of double refraction, as fluor, glass, &c. All these substances, however, act with different degrees of energy on the differently-coloured rays, according to a law which appears subject to great variations in different bodies, and which is directly deducible in any given body, from the series of tints developed by exposure to polarized light. In a former paper *, I have instanced some remarkable deviations from the ordinary law of tints exhibited by certain varieties of the apophyllite; but from the mode of experimenting there followed, the most remarkable of the peculiarities presented by the specimens employed escaped my notice. It is this, that out of the three varieties examined, two appear to belong at once to all the three classes of Media above enumerated; possessing the property of attractive crystals when exposed to the rays forming one extreme of the spectrum, and of repulsive in their action on the other extreme; while, for certain intermediate rays, they are altogether void of the property of double refraction, and allow such rays to pass freely through them *in all directions*, without dividing them into two pencils.

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[*] From the *Transact* *Philosophical Society of Cambridge*, vol. i. part ii. p. 241.

I was led to a knowledge of this remarkable singularity, by the consideration of the forms of the curves traced in Figs. 2. and 4. †, whose ordinates represent the polarizing energies of the two varieties in question, on the rays whose place in the spectrum is denoted by their abscissæ. It is assumed, without any particular grounds, in my former paper, that these curves lie, throughout their whole extent, on one side of their abscissæ. This was natural enough, being accustomed to regard crystals as necessarily included in one or the other of the great divisions above referred to, and in the mode of experimenting there resorted to, the contrary could not be discerned, the change from attractive to repulsive being marked by no phenomenon. In

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[*] *Transactions of the Philosophical Society of Cambridge*, vol. i. part i. 1820.

[†] See Plate VII. of Vol. IV. of this *Journal*, Fig. 2. and 4.

considering the subject more carefully, however, it struck me that, as the curves in question had been traced to the immediate vicinity of the axis, and, when lost sight of, were then approaching it more rapidly than in any other part of their course, it was highly probable that they would meet it in that part of the spectrum out of the reach of observation, and if so, must cut it, and the portions corresponding to the two extremities of the spectrum would thus lie on opposite sides, the absolute lengths of the ordinates given by the experiment remaining unaltered.

To put this idea to the test was a matter of considerable delicacy, as well from the imperfections of the specimen, as from its feeble polarizing energy, and the great difference of its action on the different colours. In ordinary cases, to determine whether a crystal be attractive or repulsive, nothing more is required than to place a plate of it between crossed tourmalines, so as to view the polarized rings, and then crossing it with a plate of mica or sulphate of lime, having its principal section 45° inclined to the plane of primitive polarization, to notice in which quadrants of the rings the tints are raised, and in which depressed. If the plate of the substance examined be then removed (without altering the position of the mica plate), and replaced by a plate of carbonate of lime, tourmaline, or any other known substance, (which it is convenient to keep as a standard of comparison), it is immediately seen whether the crystal in question be of the same, or an opposite character with the standard; the *corresponding* quadrants of the rings seen in the two substances being similarly affected in the one case, and the *alternate* ones in the other. So coarse a method proved, as might be expected, unavailing in the present instance, the order of the tints being so completely altered by a plate of mica of moderate thickness, as to be no longer recognised, and I was obliged to have recourse to measures taken in homogeneous light, and on a divided apparatus. The method I pursued was as follows:—Having enclosed the crystallized plate of the second variety described in my former paper, in castor oil, in a proper apparatus for varying its inclination to the polarized beam, and adjusted it so as to revolve in a plane lined to that of primitive polarization, I noticed the which the first minimum of the ordinary pencil on one side of the axis took place. These are as follows:

	Extreme Red.	Violet.
	19° 16′	25° 55′
	19 48	25 27
	19 5	25 39
	19 48	25 34
	19 5	26 31
	20 10	25 46
	19 5	25 55
	19 5	25 27
	19 5	26 54 . . near the Indigo.
	——	25 16 . . extreme Violet.
Mean	19° 28′	——
		25° 51′

The variations in the observations of the violet rays arise not so much from the imperfections of the specimen, and the difficulty of taking measures in violet light, as from the rapid change of the polarising energy, as the ray approaches the extremity of the spectrum, and the mean result above set down is, in consequence, that corresponding nearly to the mean violet rays. I now interposed between the crystal and the reflector, on which the incident light received its polarization, a plate of mica, so thin as to polarize (alone) a bluish-white of the first order; its plane being perpendicular to the ray. My object in using so thin a plate, was to dilate or contract the system of rings, by a quantity decidedly smaller than half the interval between two contiguous ones, so as to avoid all possibility of a mistake in the *order* of the ring brought under examination, by the interposition of the mica plate; the point in question being not to obtain any precise numerical results, but merely to ascertain whether the change of the inclination corresponding to the minimum or maximum of any given ring, produced by a mica plate of infinitesimal thickness, would have the same or opposite signs for the two ends of the spectrum.

The mica being fixed so as to have its principal section 45° inclined to the plane of primitive polarization (or in azimuth 45°), the minima of the ordinary pencil were now observed to take place at the following inclinations.

	Extreme Red.	Violet.	
	15° 50′	29° 59′	
	18 22	29 10	
	17 38	30 56	
	17 17	32 2	
	16 55	30 14	
	———	29 53	. . Extreme Violet.
Mean	17° 17′	31 41	
		33 29	. . Violet bordering on Indigo.
		30° 55′	

The inclination was therefore diminished for the red rays, and increased for the violet; in other words, the interposition of the mica had contracted the red rings in a direction parallel or perpendicular to its principal section, but dilated the violet.

I now (to confirm this indication) turned round the mica plate one quadrant in its own plane, so as to reverse its action on the polarised beam, and then, repeating the measures, found as follows.

Extreme Red.	Violet.
20° 53′	22° 41′
20 10	24 29
21 14	21 36
21 15	20 31
21 14	21 58
20° 57′	22° 15′

The effect on the measures, as we see, corresponds to the change of circumstances, the red rings being now expanded and the violet contracted, by the action of the mica in the direction in which they were observed. To assure myself still more completely of the identity of the ring examined, I detached the mica, and observed the first evanescence of the *extraordinary* pencil in the extreme red rays, which took place at an inclination of 27° 11′. The whole interval, then, from the maximum to the minimum of the first red ring, embraces an extent of $27° 11'-19° 28'=7° 43'$, while the displacement of the minimum by the mica did not exceed 2° 11′ in one direction, and 1° 29′ in the other. On the other hand, computing the first evanescence of the extraordinary pencil in violet light, from the formula *

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* n = the number of periods, and parts of a period, performed by the polarized ray within the plate.

l = the length of each period. t = the thickness of the plate.

θ = the inclination of the ray to the axis.

$$n\,l = t\,.\,\sin\theta\,.\,\tan\theta$$

which gives $\sin\theta = (\sqrt{2p\sqrt{1+p^2}-p})$, where $p = \frac{n\,l}{2\,t}$,

we find that it will take place at 38° 12′ of inclination, and the variations produced by the mica in the place of the ordinary minimum being + 5° 4′ and − 3° 36′, are decidedly less than the interval between the maximum and minimum of the first ring, which is 38° 12′ − 26° 19′ = 11° 53′.

However, to leave no doubt on this head, I interposed an extremely thin plate of mica, which polarized but a feeble blue of the first order, and placing its principal section successively at azimuths 45° and 45° + 90° = 135°, repeated the measures of the inclination at which the first minimum took place, taking in each case the mean of ten observations, to compensate the various irregularities it was found impossible to avoid. I thus obtained the following results.

	Azimuth — 45°	Azimuth — 135°
Inclination at Minimum of Extreme Red,	18° 39′	19° 55′
Ditto Violet, - - - -	26 15	24 35

The feeble double refraction, joined to the imperfection of the specimen, and the impracticability of cutting it into a prism, with its refracting edge parallel to the axis, (owing to the facility with which the laminæ separate from each other,) have prevented my verifying these results by actual observations on the double refraction. There can be no doubt, however, what would be the result. The spectra formed by the two refractions would appear superimposed on each other, but of different lengths, the indigo spaces of each coinciding. They would consequently appear as one spectrum of diluted colours, and unless examined in homogeneous light, the double refraction would not be noticed at all.

There can be little doubt, too, that the third variety of apophyllite described in my paper, would exhibit similar phenomena; but I have not thought it necessary to make any experiments on it, the fact which it is the object of this paper to point out, being, I hope, sufficiently proved without it.

Slough,
March 31. 1821.

Observations on the preceding Paper. By Dr Brewster.

The very important fact established with so much ingenuity and address in the preceding paper [*], appeared to me so highly interesting, that, in a paper read before the Royal Society of Edinburgh on the 17th December 1821, I have endeavoured to refer it to the ordinary influence of three positive rectangular axes, which are in equilibrium only for the yellow rays of the spectrum.

" In the *Biaxal* Apophyllite," as I have remarked in that paper, " one of the polarising axes *must be* in the plane of the laminæ, and in both the *Biaxal* and the *Uniaxal* Apophyllite, there appears to be an axis perpendicular to the laminæ. As the form of the prism of Apophyllite is perfectly symmetrical in relation to the axis, it is probable that there are two equal and rectangular axes, of a *Positive* or *Attractive* character, in the plane of the laminæ, each axis being perpendicular to the parallel faces of the crystal, and we know that there is a *Positive* or *Attractive* axis at right angles to the laminæ, and coincident with the axis of the prism [†]. The two equal *Positive* axes, which we shall call the *Horizontal* axes, on the supposition that the prism is placed upon its base, will obviously produce a single *Negative* axis, coincident with the other *Positive* axis, or the *Vertical* one perpendicular to the laminæ, and the system of rings round the resultant of these two axes, will deviate more or less from Newton's scale, according to the nature of the dispersive forces of the elementary axes [‡]. Let us suppose that the resultant negative vertical axis has the same action upon the *Yellow* rays of the spectrum as the real positive vertical axis;

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[*] A slight notice of this discovery of Mr Herschel's was given in this *Journal*, Vol. V. p. 213. See also Vol. I. p. 5.

[†] I must refer the reader to my paper " On the Laws of Polarisation and Double Refraction," in the *Phil. Trans. Lond.* for 1818, p. 231. and p. 245,—254. for the grounds upon which this resolution of axes is made. In the case of Apophyllite, there are reasons of a peculiar kind for supposing the existence of three axes.

[‡] For an illustration o[illegible] views, the reader is referred to my letter to Mr Herschel, published at th[illegible] in the *Phil. Trans.* 1820, p. 94.; and to Mr Herschel's paper in [illegible] *the Cambridge Philosophical Society*, vol. i. p. 21.; or in this [illegible]35.; and Vol. v. p. 340.

but that it acts much more energetically upon the *Red* extremity, and much less energetically upon the *Blue* extremity of the spectrum. The *yellow* rays being thus solicited by equal and opposite forces, the crystal will exercise over them no polarizing energy. The *red* rays being subjected to a greater polarizing energy from the *Negative* than from the *Positive* axis, will give rings corresponding to the difference of their opposite actions, and the characters of these rings will be *Negative*. The *blue* rays, on the contrary, being much less energetically acted upon by the *Negative* than by the *Positive* axis, will form rings proportional to the difference of their actions, and these rings will be *Positive*, from the predominating influence of the positive axis. In this way, a particular crystal of Apophyllite may exercise over the red rays of the polarized beam a negative influence; over the blue rays a positive influence; and over the yellow rays no influence at all *; while it is the general character of the mineral to exert an attractive doubly refracting force over all the rays of the spectrum. The ray of compensation, in place of being yellow, may have any position in the spectrum, and those on each side of it will afford positive or negative tints, according as the positive or the negative axis exercises over them a predominating influence.

" This view of the polarising structure of Apophyllite, affords a complete explanation of the singular tints which surround its resultant axis. Each order of colours is as it were a residual spectrum †, arising from the opposite actions of the negative and the positive axis, and the tints of which these orders are composed, will consequently vary, according to the locality of the ray of compensation."

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* This partial equilibrium of polarising forces is analogous to the paradoxical phenomena of a compound lens, which, as I have elsewhere shewn, may be constructed so as to *converge* the *Blue* rays, *diverge* the *Red* rays, and exercise *no action* at all upon the *Yellow* ones. That is, the same compound lens is a *Plane lens* in *yellow* light, a *convex* one in *blue* light, and a *concave* one in *red* light.

† Among the various residual or secondary spectra which I have examined in the course of my experiments " On the Action of Transparent Bodies upon the differently coloured Rays of Light," there are many among the polarised rings which have exactly the same tint; and there are some which resemble as nearly as possible those in Apophyllite.—See *Edinburgh Transactions*, vol. viii. p. 1.

In all the crystals which I have examined, including Apophyllites from Fassa, Utoë, Faroe, and Iceland, the doubly refracting force as exhibited by a distinct separation of the images, was *Positive* or Attractive, whether the ray was red, blue, or yellow. I therefore consider the property discovered by Mr Herschel as belonging only to some particular crystals, or parts of crystals, in which one of the three positive axes has had its intensity reduced. Specimens will, I have no doubt, yet be found, in which the Apophyllite exerts a *Negative* action on all the rays of the spectrum; for I have shewn in the paper already quoted, that the intensity of the principal axis varies in the same crystal according to a regular law, and that all its variations are symmetrical, in relation to the centre or the summits of a complete crystal. Hence, it is extremely probable, that the points or nodes at which the negative passes into the positive structure, may be found in the same crystal, and near the points *a*, *b* in Fig. 13. of my paper on Apophyllite; for in several crystals the transition at these points appeared to be very sudden.

ART. XXII.—*Notice respecting the House, &c. of Copernicus.* In a Letter from GEORGE HARVEY, Esq. Member of the Astronomical Society.

DEAR SIR,

IN a former number of your valuable Journal, you introduced a very interesting *fac simile* of one of the letters of the celebrated Copernicus. I am a lover of such fragments, because they awaken associations of a pleasing kind; and in moments of difficulty and depression, bring accessions to human happiness, more consolatory than all the pleasures which power can command.

By the kindness of a scientific friend, I have been favoured with the inclosed sketch of the house in which this illustrious astronomer resided[*]; and also of the chair in which he was accustomed to sit. The pyramidal figure near the house, is the well which supplied him with water. They have afforded me gratification, and with the view of communicating pleasure to your readers, they are now offered to your attention.

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[*] See Plate II. Figs. 5. and 6.

The astronomer, I am informed, resided in the highest garret, " a true nestling place of genius, which seems to delight in hatching its offspring in bye corners." What mingled recollections crowd on the mind, when contemplating a sketch of this kind! In this room was nursed the genius of those deep and profound speculations, which, in succeeding years, was to produce so mighty a revolution in the world. It was in this chamber that Copernicus investigated the systems of ancient times, and resolved to break through the theories which had been sanctioned by the authority of ages;—which had grown up with a long race of centuries, and become identified, as it were, with the very habits and institutions of time. It was here the native independence of his mind was displayed in its full vigour and power; and which enabled him to abandon systems that had been embellished by the purest offspring of fancy, and illustrated by talents of the most splendid order; and which seemed even in the more advanced periods of their career, to gain new accessions of strength, by becoming more intimately blended with the increasing sophistries of the schools. In this house Copernicus annihilated the labours of centuries, and prepared the elements of that mighty system, which, in an after age, was to be perfected, by the commanding genius of a Newton. This house might have often been to him an abode of uncertainty and of sorrow. Within its walls he was no doubt alternately elevated and depressed, as hope cheered him to advance, or error laid low his sanguine anticipations. During the long period on which he meditated on his noble system, his great mind must have been the frequent theatre of conflicting doubts. Fame seldom encouraged him in his career, and the vistas of hope which the vantage ground of modern science now unfolds to the strong conception of the astronomer, but seldom soothed his anxious soul.

Such men as Copernicus demand a solemn and thoughtful observation. The world but rarely sympathises with the man of close and studious retirement. How often, indeed, is he entirely neglected, till, from his lowly and unpretending solitude, there issues forth some splendid discovery to interest and instruct mankind. Yet if we trace the histories of these neglected individuals, and contemplate the influence which, in after ages, they exercise on human affairs, we shall see new reason to admire the splendid privileges of the mind, and that unbounded influence which the silent progression of time, slowly, but permanently creates, for the triumphant productions of its powers. I remain, Dear Sir, yours, &c.

PLYMOUTH,
May 4. 1822.

GEORGE HARVEY.

ART. XXIII.—*Account of a singular Experiment depending on the Polarisation of Light by Reflexion.* By DAVID BREWSTER, L. L. D., F. R. S. Lond., & Sec. R. S. Ed.

IN the course of some experiments on the polarisation of light by reflexion, I was led to observe, that a film of aqueous vapour might be breathed upon a plate of glass, without altering the distinctness of the image reflected from it surface. This effect arises from the extreme minuteness of the particles of water which compose the aqueous film; for when they are too large, which sometimes happens, the reflected image is more or less obliterated. By a little practice, however, it is easy to transfer a sufficiently attenuated film to a small plate of glass.

Let two glass reflectors A, B, Plate II. Fig. 7., be arranged in the usual manner, so that a ray of light R is incident on A, at an angle of about 56°, while the reflected ray AB is incident at the same angle upon B, so placed that the plane of reflexion from B may be at right angles to the plane of reflexion from A. When this adjustment is well made, scarcely a ray of light will be reflected from B, and the candle from which the ray R proceeds will be nearly invisible. Close to A, place another reflector of glass C, and let the ray R from the same candle be incident upon C at an angle of about 53°, while the reflected ray CD is received upon a second reflector D, (placed near B,) at an angle of 56°, and in such a manner that the plane of reflection from D is at right angles to the plane of reflection from C. As the ray R is incident upon C at an angle less than 56°, the polarising angle for glass, the reflected ray CD will not be polarised, and consequently, an image of that candle, not very bright, will be seen by reflection from D.

When an observer, therefore, looks into the two mirrors B, D, he will see no image of the candle in B, and a distinct image in D. While he is looking at these reflectors, let another person breath gently upon the glass reflectors A, C, and *the image of the candle will instantly appear in B, where there was none before, while the image of the candle that was seen in D will be extinguished.* The effect, therefore, of the same film of vapour has been to extinguish the image of the candle in D, and to create a new image in C. When the aqueous film has evaporated, which takes place very rapidly, the new image will be extinguished, and the extinguished one will reappear.

The cause of this paradoxical effect may be easily understood. When a film of aqueous vapour is laid upon the reflector A, the surface of the glass is, as it were, converted into water, and the ray RA is no longer polarised, because it is reflected from an aqueous surface, whose polarising angle is about 53°. Hence, as the ray AB is not polarised, a portion of it will be reflected from B, and will cause the candle from which it proceeds to be visible.

In the reflector C, where the ray CB is not polarised, in consequence of its being incident at an angle less than the polarising angle for glass, the substitution of a film of aqueous vapour will polarise the ray CD completely, as the ray RC is incident upon the surface of it at an angle of 53°, which is the polarising angle for water. Hence, since the reflector D is placed in the position which will not reflect a polarised ray, no part of the ray CD will now be reflected from D, and, consequently, the image of the candle, which was formerly seen, will be extinguished by throwing a film of vapour upon the reflector C.

This experiment, independent of its paradoxical character, affords a very simple and striking illustration of the relation between the refractive power of bodies, and the angle at which they polarise the incident light.

ART. XXIV.—*Account of a Free Escapement, with considerable reduction of Friction.* By URBAIN JURGENSEN, Chronometer-maker to the Danish Navy, and Member of the Royal Society of Sciences at Copenhagen *.

THE utility of exact chronometers for navigation and geography, led the British and French Governments to offer such encouragement to eminent artists, as might repay them for the time spent in the acquisition of the knowledge, both theoretical and practical, which was necessary to the advancement of an art so difficult and intricate; and they thus attained such an astonishing degree of precision, that often in successive days the error in the rate of going does not exceed fractions of seconds, or, what is the same thing, not the $\frac{1}{100000}$ part of 24 hours.

It is principally in England that the art of making exact time-keepers has been successful and general †. The numerous works of Emery, Arnold, Earnshaw, Pennington, Frodsham, Parkinson, &c. afford proofs of this remark. France is also distinguished by the excellence of the works of Bréguet, who will long be admired by those capable of appreciating the knowledge he displays, the novelty of the means he employs, and the fertility of his invention, in overcoming the difficulties of so intricate a subject.

The great exactness of chronometers depends on the execution, as well as on the principles upon which they are constructed. A great equality in the moving power, uniformity in the wheel-work, which conveys the power to the escapement, diminution of friction, and the application of means to render the latter as equable as possible, and freedom in the vibrations of the balance, are qualities indispensable to a good chronometer; but still there are others essentially necessary to its perfection, namely, the true relation between the movement of the regulator and the moving-power, a rigorous exactness in the compensation for the variation of temperature, and perfect isochronism in the vibrations of the balance, as obtained by the spiral. The great regularity of its going, principally depends on the success in obtaining these, abstraction being made of the quality of the oil employed.

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* Translated and abridged from Shumacher's *Astronomische Nachrichten*, No. x. p. 155. March 1822.

† It is agreeable to observe, that the superior pretensions of British artists, in the construction of chronometers, which were so strongly and ably stated in the *Edinburgh Review* for Octo[illegible] xxii. p. 372. are asserted under a still more formidable contrast [illegible] whose judgment must be considered as at least disinterested.

The principles on which compensation for temperature depend, are so well established, that great exactness is attainable, and a want of success in obtaining a just compensation can only be imputed to a want of attention in the artist.

Perfect isochronism in the vibrations of the balance is more difficult to attain than the compensation, and requires more trials and care; but, although it is one of the parts most essential for continued regularity, machines have been known to go well without the isochronism being so rigorous as the compensation.

Still, since a chronometer cannot be considered perfect without it, every care must be taken to succeed. The two modes of constructing the balance-spring, so that the great and small vibrations of the balance may be equal, are well known, and even the general theory of isochronism is thoroughly established, but still much tact and nicety are necessary for success, and on this point genius will do more than rule.

The escapement, that delicate part of a chronometer, deserves and requires particular attention. The escapement, with a *constant force*, promises, by the theory, the greatest regularity, for in it the impulses which the regulator receives, are always of equal force, and the resistance made to the regulator in disengaging the detent is also constantly the same; hence there is nothing to disturb the regularity of the vibrations which, from the nature of this escapement, become always of equal extent. The late *Thomas Mudge* used this escapement in his chronometers, and their goodness in an equable temperature is no doubt in a great measure owing to it. Bréguet also employed such an escapement, but more perfect, in many of his chronometers, and particularly in the celebrated one lately made for the Count Sommariva; but this escapement, in spite of its advantages, can never become of general use, because it requires, in the execution and in the combination, precautions often beyond the abilities of even good workmen. However, were we still ignorant of the mode of making the vibrations of the balance isochronous by the nature of the spiral, the artist would be obliged to submit to all the trouble and care required in the execution of the escapement with a constant force.

The free escapement of Earnshaw is at present the one chiefly used in the construction of chronometers for navigation, as it produces a great regularity, and is not very difficult in the execution. It is preferable to Arnold's, from having less friction, being less liable to be stopped, and being less difficult in the construction. Its superiority is proved by its being employed by the best artists, and by the British Government having awarded L. 3000 to Mr Earnshaw for its invention.

The means used by Mr Earnshaw to diminish the friction of his escapement, consists,

1st, In giving a greater diameter to the circle of escapement, which causes the escapement-wheel to act by a longer lever, and more perpendicularly against the line of centres. The push thus becomes shorter and more gentle, and operates with less play, and consequently with less friction.

2dly, In supporting the detent at the extremity of the teeth of the escapement-wheel, by which disposition, the pressure of the teeth of the wheel against the detent acts by the longest possible radius, consequently with less pressure than in the escapement of Arnold, and thus the regulator also has less friction in disengaging the detent. A chronometer with an escapement of Earnshaw's, admits a balance both greater and heavier than one with an escapement of Arnold's, and, therefore, there will be a greater quantity of motion with the same moving power, an evident proof of a reduction of friction.

Notwithstanding these advantages, the author of this paper conceives, that the diminution of friction in that escapement may be carried much farther ; and a model on a large scale, executed with that intention, proves that this advantage is obtained without any inconvenience. It would be tedious to describe in detail this escapement, which could only be made intelligible by a plan both of the front and side of it ; but for those who are acquainted with the [illegible] f Earnshaw, it will be sufficient to say, that instea [illegible] ement wheel he employs two upon the same a [illegible] se acts by impulse on the escapement circle, of [illegible] eter is in a still greater ratio to the diameter of the wheel, than it is in that of Earnshaw. The other wheel, of which the diameter is almost double that of the impulse wheel, produces the pause to allow the regulator to accomplish freely its vibrations and its rests, by the extremity of its teeth on the detent-spring. Since this wheel is of a much greater diameter than the impulse-wheel, it is evident that the pressure of the wheel work against the detent will be weaker than in the escapement of Earnshaw, when the same wheel serves for the impulses and the pause; by this diminution of pressure, the friction of the teeth against the detent will be reduced, and the regulator will thus suffer less resistance in disengaging the detent-spring from the stop-wheel.

In Earnshaw's escapement, since each tooth of the escapement-wheel has a double operation to perform during a revolution, it must be made very thick, in order that the fine points of the teeth may not wear off. In the free escapement with a double wheel, where the teeth have only one operation, the wheels admit of being made lighter, so that their inertia will be reduced nearly to the same quantity as that of the single, but thicker wheel.

The great reduction of friction, particularly at the escapement, will undoubtedly be of real advantage, since gentle frictions are always more constant than those which are heavy, that is less subject to be augmented, particularly in cases where the use of oil must be avoided. And it is this circumstance which gives us reason to hope that the escapement now proposed may contribute to the precision of chronometers.

ART. XXV.—*On the Formation of Valleys, Bays, and Creeks.*

By the Rev. GEORGE YOUNG, M. A. Whitby.

MR YOUNG, in his valuable Geological Survey of the Coast of Yorkshire, just published, which we recommend to the attention of geologists, advocates that opinion which refers many of the inequalities on the earth's surface to breakings and changes of position in the strata, and to denudations or washing away of large portions of the upper strata. In the following observations, he shews how this mode of explanation harmonises with geognostical phenomena in Yorkshire.

Our vales, river-courses, bays, and creeks, says he, have been formed chiefly by breaks and denudations. Some eminent geological writers have maintained, that the valleys have been formed by the streams which flow in them; that every river has hollowed out its own channel in the hills through which it passes; that bays and islets on the shore have been excavated by rivers, and by the action of the sea; and that the alluvial beds, even in the highest situations, have been deposited by rivers, that once flowed there. The phenomena which our district exhibits, can by no means be reconciled with such notions. The agency of rivers has indeed had a considerable effect in modifying their channels, but the channels have derived their origin from breaks and denudations of the strata; to which also the indentations of the coast are principally owing. This is so much the case, that we observe some dislocation of the strata, at almost every bay and inlet of the coast, and at the mouth of almost every stream that runs into the ocean. The Esk did not cut its way into the sea through our rocky cliffs, but its channel was opened by a vast break in the strata, and by the denudation or washing away of the loose materials, disengaged by the shock which this dislocation produced. The river may have filled up some hollows and inequalities of its original channel, but it cannot have excavated those extensive and diversified dales through which it runs. This is obvious from the circular shape of some of them, as that of Ruswarp; from their general form and arrangement; and from decisive indications of breaks, elevating the strata on the south side, and sinking those on the north. The numerous dales that open into the vale of the Esk from the south, have originated in an equal number of fractures in the strata, aided by denudations of these strata; and, in some instances, as at Fryop and at Danby, there has been a compound fracture, the dale, which is single at its upper end, branching into two before it reaches the Esk, and inclosing between the branches a delta-shaped hill, detached from the principal chain. This phenomenon, together with the steepness of the sides of this dale, and the roundness of their upper extremities, must wholly set aside the i[illegible]hat they could be hollowed out by the petty rivulets w[illegible] through their rich alluvial bottoms. As a proo[illegible]s not hollowed out its bed, we may also notice, that a little below Danby Castle, it turns aside to the right, out of what appears its direct course in the valley, and passes through Cronkley Gill, in a deep rocky chasm, the margins of which are much higher than the middle of the vale, through which we might have expected the river to flow, and into which it again returns at Lealholm Bridge. Now, if the river had excavated the middle part of the valley, and formerly flowed there, how could it forsake that natural channel, to cut a circuitous passage through rocky strata, the surface of which is considerably higher than the alluvial vale which, on that supposition, it deserted? A similar instance occurs in that small branch of the Esk, which flows from Tranmire and Stonegate. A little below Stonegate the stream bends to the right, and runs in a deep rocky fissure on one side of the alluvial valley; the middle part of which is left dry, though greatly below the level of the banks of the stream, as if artificial means had been used to turn the rivulet out of its natural course. A third instance, fully more remarkable, occurs in the Esk, near where this stream joins it. The river, before arriving at Arncliff, leaves the valley, and seems to make a sweep through part of a hill. In a similar way, the Derwent, which rises in the moors between Whitby and Scarborough, does not run down to the sea by the opening at Scalby, but passes on between steep banks to Ayton, and into the vale of Pickering; and, on approaching the chalk strata, it does not turn to the left in the direction of Filly, which seems the most natural and easy passage, but flows round the foot of the Wolds, by Malton and Howsham, finding its way between banks that are higher and stronger than the obstacles which opposed its progress in the other direction. In such cases, it is quite clear that the streams have neither cut their channels, nor deposited the alluvium that covers the principal part of the valleys through which they pass. Many of the valleys, indeed, bear marks of their having been formed or modified by the flowing of waters; yet not by the flowing of their own little streams, but by the waters which once covered the whole of our rocks, and which, in retiring, have washed away vast portions of the more loose or exposed parts of the strata, and employed the materials, thus obtained, in forming the alluvial covering.

In like manner, the River Tees, and its tributary streams, cannot be conceived to have given the fronts of the Cleveland Hills their shape, and formed the deep recesses between them. The Leven, which collects a great part of the streams that flow from them, makes its way through deep beds of alluvium, which it cannot have deposited; but though it has deepened its channel, by wearing away the soft alluvial beds, it has made little impression on the sandstone strata, in the few places where it reaches them. To ascribe to currents so limited in their operations, effects of such magnitude as the shaping of mountains, and the formation of extensive valleys or plains, is altogether unphilosophical.

Besides, what shall we say of those valleys in which there are no streams, of which we have so many in our oolite hills and chalk hills? And what shall we say of the subterraneous channels of the becks that flow into the vale of Pickering? Have these channels also been excavated by the streams? Or how shall we account for another singular fact, that the becks or rivulets which flow southward from the highest range of the alum hills, on arriving at the oolite hills, pass directly through them, instead of turning into the valley that runs along their northern fronts? Had a river run in that valley, all these becks would have fallen into it, and pursued their course with it to the ocean, without breaking through the oolite hills at all; or, at the most, one passage through these hills, would have served for the whole. When, therefore, we see each of these becks holding a straight course through an opening in the oolite hills, exactly opposite its channel in the alum hills, there is no way of explaining the phenomenon, but by supposing that both parts of its channel have been formed by the same break in the strata.

Another proof that river courses have been produced by such breaks and denudations, we find, in those places where the heads of two opposite valleys run into one, their heads forming a low marsh, from whence the waters flow in contrary directions. Thus, Newton Dale is connected with the Vale of Godeland by a narrow marshy vale or hollow, above a mile long, called the *Fen*. This fen, in which the waters are stagnant, has the same kind of high banks as the dales which it unites, and of which it is obviously the continuation. Now, if we could suppose, that the beck of Newton Dale has cut its channel, a supposition highly improbable, the channel being extremely deep, and carried through thick beds of crow-stone and other hard rocks, the question would remain to be solved, What hollowed out the fen? It is clear, that the formation of the fen, and of the dales which it unites, must be attributed to one great break, passing from the Vale of Pickering into that of the Esk. An instance of the same kind occurs in the two opposite vales of Commondale and Kildale, which form but one long, narrow valley, connecting the vale of Esk with the plain of Cleveland. As the whinstone dike passes through part of this valley, the idea of its being the result of an extensive break is the more confirmed.

In regard to the indentations of the shore, there is no doubt that the sea has greatly modified their shape, but their origin must be ascribed to breaks and denudations of the strata. Wherever bays have been formed on the coast, as at Filey, Robin Hood's Bay, Sandsend, and Runswick, we can trace their formation to breaks, subsidences, or interruptions, of the strata. Even our promontories and cliffs must have been formed by similar means. Vast masses of the strata must have sunk down, and become part of the bed of the ocean, while the rocks from which they were detached have remained as sea-cliffs. How can we account for our submarine cliffs and promontories, but by supposing that the rocks, which are below the level of the sea, have experienced dislocations, like those above it? Indeed, the form and position of Whitby Rock may suffice to show, that the breaks in the strata have extended outwards beyond the line of the shore."

ART. XXVI.—*Account of the Eruption of the Old Volcano of Eyafjeld Jokkul in Iceland, in December* 1821.

THE remarkable fall of the barometer which took place almost simultaneously throughout all Europe, on the 25th of December 1821, and which in some cases was accompanied with an agitation of the magnetic needle, induced many persons to conjecture that some tremendous convulsion of nature must have visited some part of the globe. This conjecture has at last been verified by a volcanic eruption of the old volcano of Eyafjeld Jokkul, which has been in a quiet state since the year 1612.

This mountain, otherwise called Cape Hecla, is about 5666 feet in height. It is nearly equidistant from Kolla and Hecla, and is the southermost of the chain where the dreadful eruption broke out about the middle of the last century.

On the 19th December 1821, the eruption began. The crater was formed at the distance of five miles, from the minister's house at Holt, and discharged itself through the thick mass of ice that enveloped it, and which is seldom melted. The ice was dispersed in every direction, and a mass, 18 feet high and 60 feet in circumference, fell towards the north. A number of stones, of different sizes, rolled down the mountain, accompanied with a noise like thunder; and this was immediately followed by a discharge of an enormous and lofty column of flame, which illuminated the whole country, and allowed the people at Holt to read as perfectly within their houses at night as if it had been day. Ashes, stones, gravel, and heavy melted masses of rock, some of which weighed about 50 lb. were thrown up, and one of these last was found at the distance of five miles from the crater. On the day immediately following the eruption, a great quantity of the fine greyish-white powder of pumice * was discharged, and carried about by the wind so as to fall like snow, and cover the adjacent country. It penetrated into the houses through every opening. It exhaled a disagreeable smell of sulphur, brought on affections in the eyes, and occasioned diseases among the sheep in Vester Eyafjeld and Oster Landoe.

On the 25th of December, a violent storm raged from the south, and by the united action of the wind and the rain, the fields were cleared of the sulphureous dust which had covered them. On the 26th and 27th of December, there was a heavy storm from the north-east, and the barometer, which had been gradually falling since the 18th December †, when it was 29°.16,

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* This powder had a sulphureous taste, and burned when thrown into the fire.

† The observations or [illegible] ter were made at Naes near Reikavig, by Dr Thorsteinson. A stat[illegible] ber, January, and February, will be found in the *Ann. of Phi*[illegible]

had reached, on the 26th December, its lowest point at 28°49. It is a curious fact, however, that on the 8th of February the barometer fell to 27.25, a time when no earthquake was felt, and no apparent change had taken place in the eruption. On the 18th of February, the barometer, which had been at 29.42 on the 11th, fell to 27.72. So late as the 23d of February, the Eyafjeld Jokkul emitted smoke greatly resembling the steam of boiling water; and some persons were of opinion that the mountain had decreased, and was lower near the crater, as it evidently appeared to be when viewed in a direction from north to south.

It is stated that the water in the rivers that flow from the Jokkul and the surrounding mountains, had been considerably enlarged during the first day's eruption. A constant rumbling noise was heard in the vicinity of the volcano, attended occasionally by a dreadful crash, as if the immense masses of stones and ice were on the eve of being precipitated down the mountain.

Other two volcanoes to the east, in the mountains of Kolla and Oraefa Jokkul, are said to have broken out, but no certain information has been received on the subject.

The vessel which brought the account of the volcanic eruption to Copenhagen, left Iceland on the 7th of March; and it is reported that the sailors when at sea, again saw a violent fire in the direction of the volcano.

ART. XXVII.—*Analysis of a Paper on the Finite Extent of the Atmosphere.* By W. H. WOLLASTON, M. D. V.P.R.S *.

IF air consist of ultimate particles, whose divisibility has a limit, an atmosphere composed of such particles must have a finite extent, because it cannot expand beyond that distance at which the force of gravity upon a single particle is equal to the resistance arising from the repulsive force of the medium. In order to ascertain whether or not this is the constitution of our atmosphere, Dr Wollaston considers what would be the effect of an unlimited expansion of an atmosphere, and finding that no such effects are exhibited in any of the bodies of the planetary system, he concludes that these bodies have not an atmosphere of indefinite extent; that the earth's atmosphere is also limited, and consequently, that matter has a finite divisibility, and that the doctrine of ultimate atoms is thus indirectly established.

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* Read before the Royal Society on the 17th January 1822.

If the expansion of any atmosphere is unlimited, the same kind of matter must pervade all space, and the sun, moon, and all the planets must have this matter condensed around them in quantities dependent on the force of their respective attractions. For the purpose of determining if such an accumulation of matter does exist round any of the planetary bodies, he begins with the *Sun*, which, on account of its having the greatest mass, ought to accumulate round it the greatest quantity of atmospherical matter.

Assuming the sun's mass as 330,000 times that of the earth, and his radius 111.5 times that of the earth, he finds that the distance from the sun's centre at which his atmosphere will have a density fully equal to our own, and therefore capable of refracting a ray of light more than one degree, is $=\sqrt{330,000}=575$ times the earth's radius, $=\frac{575}{111.5}=5.15$ times the sun's radius; that is, a point whose angular distance from the sun's centre is $15' \, 49'' \times 5.15 = 1^\circ \, 21' \, 29''$.

Now, if any of the planets or stars, in approaching the sun's disc, suffer no refraction at all, when carefully observed at the above distance, or at less distances from the sun's centre, we may safely conclude that no such atmosphere exists.

In order to determine this point, Captain Kater made a series of observations on Venus on the 18th and 19th of May 1821, when she was on the eve of her conjunction with the sun, and Dr Wollaston made similar ones after her conjunction. Captain Kater's last observation was made when Venus was only 65′ 50″ from the sun's centre; and Dr Wollaston's when her distance was only 53′ 15″; and at both these times, neither her motion nor her position were in the least affected by a solar atmosphere. In 18[illegible] [illegible]al of Montpellier observed *Venus* when her distanc[illegible] [illegible]e was only 46′, and Mercury when his distance was only 65′; and in both these cases, the observed and the calculated positions agree exactly.

These arguments receive, if they required it, additional strength from the phenomena of the eclipses of Jupiter's satellites. These bodies advance regularly, and without any retardation from refraction, to the very disc of the planet; so that Jupiter cannot possess that extent of atmosphere which he is capable of attracting to himself from an infinitely divisible medium filling space. For, taking Jupiter's mass at 309 times that of the Earth, and his diameter at 11 times that of the Earth, then $\sqrt{309} = 17.6$ times the Earth's radius, $= \frac{17.6}{11} = 1.6$ times his own radius, which will be the distance from his centre at which an atmosphere equal to our own should produce a refraction of one degree. To the fourth satellite this distance would subtend an angle of about 3° 37′; so that an increase of density to 3½ times our common atmosphere, would be more than sufficient to render the fourth satellite visible to us *when behind the centre of the planet, and consequently, to appear on both* (or all) *sides at the same time.* The space of about 6 inches in depth within which this increase of density would take place, would not subtend so much as $\frac{1}{300}$th of a second.

Hence, Dr Wollaston concludes, *that all the phenomena accord entirely with the supposition that the Earth's atmosphere is of finite extent, limited by the weight of ultimate atoms, of definite magnitude, no longer divisible by repulsion of their parts.*

Having thus stated Dr Wollaston's reasoning as perspicuously as we can, we shall lay before the reader, in his own words, a description of the apparatus which he employed.

"If I were to describe the little telescope with which my observations were made, without taking due care to explain the precautions adopted, and the grounds of their efficacy, it might perhaps be scarcely credible, that, with an object-glass less than one inch in aperture, having a focal length of only seven inches, I could discern an object not to be seen by telescopes of four and five inches aperture. We know, however, that this small aperture is abundantly sufficient for viewing Venus at a distance from the Sun; and since the principal obstruction to seeing her nearer, when the atmosphere is clear, arises from the glare of false light upon the object-glass, the success of the observation depends entirely on having an effectual screen for the whole object-glass, which is obviously far more easy to accomplish in the smaller telescope.

"Since the screen which I employed was about six feet distant from my object-glass, a similar protection for an aperture of five inches would have required to be at the distance of thirty feet, to obviate equally the interference of the sun's light at the same period; but this is a provision with which regular observatories are not furnished for the common purposes of astronomy.

"As I hope at some future time to avail myself of a larger aperture for such observations, without the necessity of mounting a more distant screen, it may be desirable that I should suggest to others the means by which this may be effected, if they think the question of a solar atmosphere worthy of farther investigation. If an object-glass of four inches aperture be covered so as to expose only a vertical slit of its surface one inch in width, the surface of glass to be so used is about five times as large as the circular aperture, one inch in diameter, and yet will be as completely shaded by a vertical screen at any given distance; and an interval of only five feet might allow a star or planet to be seen within a degree of the sun's disc.

"When the sun and planet have the same declination, the vertical position of the slit is manifestly the most advantageous that could be chosen on the meridian; but, for the purpose of seeing to the greatest advantage when the line of the centres is inclined to the horizon, it would be requisite to have the power of turning the slit and screen together, at right angles to any line of direction of the centres.

"The only fixed star sufficiently near to the ecliptic, and bright enough to give any prospect of its being seen near the sun, is Regulus, which passes between the 20th and 21st of August; but I have not yet had an opportunity of ascertaining within what distance from the sun this star can be discerned."

ART. XXVIII.—*Magnetical Observations made at Bushey Heath, near Stanmore.* By Colonel BEAUFOY, F. R. S. Communicated by the Author.

MAGNETICAL OBSERVATIONS, 1822.—Variation West.

MONTH.	Morning Observ.		Noon Observat.		Evening Observ.	
	Hour.	Variation.	Hour.	Variation.	Hour.	Variation.
March 1.	8h 33′	24° 32′ 47″	1h 30′	24° 37′ 46″		
2.	8 30	24 26 42	1 30	24 40 40		
3.	8 30	24 31 30				
4.	8 28	24 30 28	1 38	24 37 13		
5.	8 30	24 28 18	1 34	24 36 51		
6.	8 34	24 27 17	1 30	24 37 08		
7.	8 28	24 28 04	1 30	24 38 05		
8.	8 32	24 27 06	1 30	24 36 55		
9.	8 36	24 27 24				
10.	8 32	24 26 43	1 30	24 35 43		
11.	8 35	24 27 24	1 40	24 34 20		
12.	8 37	24 27 37	1 25	24 36 17		
13.	8 40	24 31 30	1 26	24 38 11		
14.	8 35	24 27 45	1 25	24 36 30		
15.	8 35	24 30 19	1 25	24 36 22		
16.	8 25	24 31 50	1 16	24 36 47		
17.	8 32	24 27 36	1 25	24 35 24		
18.	8 30	24 26 40	1 25	24 36 58		
19.	8 35	24 27 54	1 25	24 35 07	6h 05′	24° 32′ 12″
20.	8 30	24 26 29	1 27	24 36 02	6 15	24 31 30
21.	8 35	24 28 26	1 35	24 37 55	6 25	24 27 42
22.	8 30	24 24 35	1 25	24 35 09	6 18	24 25 38
23.	8 30	24 25 53	1 30	24 36 49	6 20	24 28 48
24.	8 30	24 26 26	1 25	24 35 11	6 20	24 28 39
25.	8 35	24 25 23			6 25	24 27 29
26.	8 30	24 24 45				
27.	8 30	24 25 25	1 47	24 38 15	6 25	24 30 20
28.	8 32	24 25 55	1 34	24 35 26	6 13	24 28 58
29.	8 28	24 26 30	1 28	24 36 13	6 20	24 28 14
30.	8 40	24 26 15	1 45	24 35 46	6 27	24 27 54
31.	8 35	24 25 46	1 30	24 35 26	6 30	24 27 33
Mean for Month,	8 32	24 27 38	1 29	24 36 36	6 20	24 28 45

By comparing the mean of these observations with those of March 1819, the period of the greatest Western Variation, the Morning decrease is 5′.40″, the Noon 5′.06″, and the Evening 6′.32″; Mean of the three 5′.40″, which is an annual diminution of 1′.55″.

ART. XXIX.—*Celestial Phenomena, from July 1. to October 1. 1822, Calculated for the Meridian of Edinburgh, Mean Time.* By Mr GEORGE INNES, Aberdeen.

JULY 1822.

D.	h	′	″	
1.				☉ in perigee.
3.	9	50		A. ☌ ☽ ♅
4.	2	30		F. ☌ ☽ φ ♐
	10	45		F. ○ Full Moon.
6.	1	10		F. ☌ ☽ β ♑
11.	11	0		A. ☾ Last Quarter.
13.	8	30		F. ☌ ☽ ♄
14.	4	40		F. Inferior ☌ ☉ ☿
	5	30		A. ☌ ☽ ♃
15.	1	20		F. ☌ ☽ χ ♉
	6	40		A. ☌ ☽ ♀
17.	0	20		F. ☌ ☽ μ ♊
	1	15	40	F. Im. III. sat. ♃
	3	21	0	F. Em.
18.	1	54		A. ● New Moon.
	3	10		F. ☌ ☽ ☿
20.	8	20		A. ☌ ☽ α ♌
23.	1	22	13	F. Im. 1. sat. ♃
	Noon			☉ enters ♌
	4	20		A. ☌ ☽ ♂
25.	10	40		A. ☽ First Quarter.
28.	9	20		A. ☌ ☽ α ♏
31.	2	30		☌ ☽ ♅

AUGUST 1822.

D.	h	′	″	
2.	10	44	51	A. Eclipse ☽ begins.
3.	0	10	24	F. Ecliptic ☍
	0	17	6	Middle.
	1	49	22	End, digits eclipsed, 9° 3′ 42″ on ☽'s N. limb.
4.				☿ Greatest Elongation.
6.	1	8	42	F. Im. II. sat. ♃
	3	45	41	Em. ——
9.	4	40		A. ☌ ☽ ♄
10.	4	10		F. ☾ Last Quarter.
11.	9	20		F. ☌ ☽ ♃
14.	11	45		F. ☌ ☽ ☿
15.	1	31	38	F. Im. I. sat. ♃
16.	0	5		F. ☌ ☽ ☿
	11	8		A. ● New Moon.
19.	9	20		A. ☌ ☽ η ♍
21.	11	22	10	A. Em. III. sat. ♃
22.	3	25	10	F. Im. I. sat. ♃
23.	3	0		F. ☌ ☽ α ♎
	6	22		A. ☉ enters ♍
24.	3	55		A. ☽ First Quarter.
27.	10			F. ☌ ☽ ♅
28.	7	20		F. Sup. ☌ ☉ ☿
29.	1	22	18	F. Im. III. sat. ♃
	3	14	40	Em. ——
30.	11	47	2	A. Im. sat. ♃

SEPTEMBER 1822.

D.	h	′	″	
1.	0	52	16	F. Em. II. sat. ♃
	0	13		A. ○ Full Moon.
5.	9	40		A. ☌ ☽ ♄
6.	1	8	42	F. Im. II. sat. ♃
7.	3	50		F. ☌ ☽ η ♉
	5	0		A. ☌ ☽ ♃
	7	45		A. ☌ ☽ α ♉
8.	9	7		F. ☾ Last Quarter.
9.	9	30		A. ☌ ☽ χ ♊
0.	9	50		A. ☌ ☽ β ♊
3.	9	50		A. ☌ ☽ ♀
4.	3	31	51	F. Im. II.
	3	34	5	F. Im. I. s
5.	10	45		F. ● New

D.	h	′	″	
15.	10	2	29	A. Im. I. sat. ♃
16.	5			A. ☌ ☽ ☿
19.	9	30		F. ☌ ☽ ♂.
20.	10	30		A. ☌ ☽ δ ♏
	11	30		A. ☌ ☽ η ♏
	11	40		A. ☌ ☽ β ♏
22.	11	55	58	A. Im. I. sat. ♃
23.	10	30		F. ☽ First Quarter.
	2	50		A. ☉ enters ♎
	5	20		A. ☌ ☽ ♅
26.	1	15		F. ☌ ☽ β ♑
	1	49	34	F. Im. I. sat. ♃
	11	5		A. ○ Full Moon.

Explanation of Abbrevi [illegible] Forenoon; A. Afternoon; ☌ Con-*junction*; ☍ Opposit [illegible] ; Em. Emersion; sat. Satellite.

ART. XXX.—*Proceedings of the Royal Society of Edinburgh.* (Concluded from Vol. VI. p. 372).

March 18. 1822. MR P. F. TYTLER read the continuation of his "*Biographical Sketches of some of the Earliest of our Scottish Lawyers.*"

April 1. A paper was read by Richard Saumarez, Esq. entitled, "*An Inquiry into the Nature of Atmospheric Pressure.*"

April 15. A paper by Dr Brewster was read, entitled, "*Description of a Monochromatic Lamp, with observations on the Composition of different Flames, as modified by Reflexion, Refraction, and Combustion.*"

The chief object of this paper was to describe the principles and construction of a *Monochromatic Lamp*, for illuminating objects with a homogeneous flame, which the author succeeded in constructing, after many unsuccessful attempts. By illuminating microscopic objects with this lamp, a distinctness and perfection of vision was obtained, which extends widely the power of the microscope, and enables it to detect delicate structures, and minute organizations, which are beyond the reach of observation when common light is employed. The author pointed out the application of this lamp to various purposes, both practical and scientific, and particularly to the measurement of minute optical phenomena, such as those of refractive powers, double refraction, and polarisation, and the phenomena of periodical colours. As the yellow light discharged from this lamp has an invariable character, the measures of these and other phenomena, taken in such a light by different philosophers, may now be referred to as an unchangeable standard, and they will also have the advantage of being made in the most luminous rays of the spectrum, and of being referable to rays that have nearly a mean refrangibility.

The author likewise pointed out the manner in which the prismatic spectrum is attacked, and finally extinguished, by the action of differently coloured absorbing media; and he has established, in opposition to the deductions of Dr Wollaston and Dr Young, that the yellow light has a separate and independent existence in the solar spectrum.

On the same evening, a paper by Captain Scoresby *junior* was read, entitled, "*Observations on the Errors in the Sea-Rates of Chronometers, arising from the Magnetism of their Balances; with suggestions for removing this Source of Error.*" In this paper, Mr Scoresby suggests three means of lessening or obviating altogether the anomalous action of the Magnetism of the balance.

1. To employ a substance in the construction of the balance without magnetical properties.
2. To free the balance of any magnetism accidentally acquired.
3. To prevent the unequal action of the magnetic influence, by giving to the chronometer a fixed position, as regards the magnetic meridian.

In order to apply the first method, he proposes to make the balance of platinum. In order to accomplish the second object, he suggests that the flat surface of the balance be ground and polished in the plane of the magnetic equator. To effect the third object, he makes the chronometer traverse upon the plate of a compass-needle; and he has found this contrivance quite practicable, the magnet which directs the plate being at such a distance as not to affect the chronometer.

May 6. A paper was read, containing a series of Electro-magnetic Experiments and Observations, by T. S. Traill, M. D. and Captain William Scoresby.

May 20. An account of the Analyses of the Faroe Apophyllites, and other minerals, by M. Berzelius, was read, and also Observations on this paper, and on the Optical Analysis of Minerals, by Dr Brewster. These two papers are printed in this Number, p. 1. and 11.

June 3. The following gentlemen were elected Members of the Society:

FOREIGN.

Baron Larrey.

ORDINARY.

James South, Esq. F. R. S.
Lieutenant-Colonel Martin Whyte.
W. F. Campbell, Esq. of Shawfield, M. P.
Geo. Jos. Bell, Esq. Prof. of Scots Law.
Dr William Dyce, Aberdeen.
W. C. Trevelyan, Esq.

At the same meeting, a paper by Henry Mackenzie, Esq.

was read, entitled, " *Hints on the subject of Dreaming, and on the Operations of the Mind in Dreaming.*"

The following Donations have been presented to the Society:

The *Transactions of the Horticultural Society of London*, 4 vols. 4to. Presented by the Society.

Specimens of Osseous Breccia found in levelling the ground near to Rosia in Gibraltar, for the purpose of erecting a ball court for the soldiers. Presented by Lieutenant Macniven of the 26th Regiment.

Memoirs of the Cambridge Philosophical Society, vol. i. part ii. Presented by the Society.

Additional Numbers of the *Flora Batava*. By his Majesty the King of the Netherlands.

Memoirs of the Astronomical Society of London, vol. i. Presented by the Society.

Professor Shumacher's *Astronomische Nachrichten*. Presented by the Author.

Translation of Berzelius's Work on the Blowpipe. Presented by J. G. Children, Esq.

A part of the Book-Case of Sir Isaac Newton. Presented by John Robison Esq.

Art. XXXI.—*Proceedings of the Wernerian Natural History Society.* (Continued from Vol. VI. p. 374.)

1822, *Feb.* 9. THE Secretary communicated the substance of a memoir by Mr David Don, curator of the Lambertian Herbarium, on a small family of plants, which he has called *Didymocarpeæ*. (This memoir is printed in the present number of this *Journal*, p. 82. *et seq.*) He also read descriptions of two remarkable varieties of *Nuphar lutea*, or the yellow water-lily, found in the Corby Loch, near Aberdeen, communicated by Mr William Macgillivray.

Professor Jameson then read the second part of M. Boué's paper on the *Igneous Origin of Granite and Trap-rocks.*

Feb. 23. Mr R. K. Greville read the first part of a paper entitled, " *A new arrangement of the Genera of Mosses, accord-*

ing to their natural Affinities." In these muscological inquiries, Mr G. W. Arnott is associated with Mr Greville.

Professor Jameson then read a *Geological Account of the country in the neighbourhood of St John's, Newfoundland*, by Mr John Baird, illustrated by sketches, shewing the arrangement of the rocks. The Professor also communicated a short paper on the Causes of the non-existence of Human Fossil Remains in Europe, by Mr Murray of Simprim.

Mar. 9. The Secretary read a communication from the Rev. James Grierson, M. D. minister of Cockpen, *On the Natural History and Habits of the Talpa Europæa, or Common Mole.* At the same meeting was read an *Account of the Phænogamous Vegetables and of the Birds observed along the banks of the river Dee, from its sources in Bræmar to its embouchure at Aberdeen*, by Mr Macgillivray.

March 23. The Secretary read two communications from Lawrence Edmondston, Esq. of Unst in Shetland; the first describing a specimen of the Snowy Owl, *Stryx nyctea*, with some account of its habits; the second giving an account of a specimen in perfect feather, of the Iceland Gull, of which he had formerly shot a bird in immature plumage. Both specimens were now exhibited at this meeting, and, from the external characters, combined with the peculiar manners of the bird, Mr Edmondston concluded that it is a distinct species, and proposed to call it *Larus Islandicus.*

Professor Jameson then communicated to the Society some observations on the natural history of Fossil Organic Remains, as connected with the theory of the earth.

April 6. Professor Jameson read a notice from Captain Scoresby, regarding recent experiments made by him on the remarkable effects of Percussion on Steel, in producing strong magnetic powers. The Professor also read an account of M. Tiedemann's comparisons of the Brain of Man with that of Apes, Seals, Whales, Bats, and other animals of the Class Mammalia. The Secretary then read a notice regarding the Northern and Speckled Divers, [illegible] Lawrence Edmondston.

April 20. Pro[illegible] read a notice
Tusk of a Fossil [illegible] mmoth dug
shire, illustrated [illegible] the tusk;

Mr James Hollway of Boston. He then gave an account of the Primitive Strata which occur near Lochgilphead, illustrating his description by sketches or plans. The Professor likewise read extracts of a Letter from Dr Oudney, now on an exploratory excursion in Africa, dated Tripoli, 24th January last.

The Secretary then read a Notice of a remarkable Hail-storm in Essex, communicated by a gentleman who witnessed it.

Likewise extracts of a Letter from Mr Strang of Lopness, in Orkney, describing some singular effects of the violent gales in that quarter during the past winter.

He also laid before the Society a thermometrical register, taken every hour of the day and night, between 6 A. M. of 1st April and 6 P. M. of 6th April, by Mr Coldstream of Leith. The general results were, That generally the *minimum* was at 6 A. M. and the *maximum* at 6 P. M.; that the average rise was 2°.03; the fall 2°.012; that in the night of the 4th and morning the 5th April, the temperature was stationary for five hours; and that the average temperature of each day was the same nearly as what was observed at 8 A. M. and 8 P. M.

At this meeting the following gentlemen were balloted for and duly admitted:

Resident Members.

John Stewart, Esq.	John Anderson, Esq. younger of Gladswood.
Robert Scott, Esq.	Robert Hamilton, M. D. F. R. S. E. &c.

Non-Resident.

John Richardson, Esq. now on the Arctic overland Expedition.

Foreign.

Dr H. F. Autenreith of Tubingen. H. Bowdich, Esq. Paris.

Art. XXXII.—*Proceedings of the Society of Scottish Antiquaries.*

THE following papers have been read before this Society in the course of the winter 1821 and 1822:

1. Illustrations of the Manorial Tenures and Customs of the north of England in the fifteenth century, derived from original documents, and communicated by Dr Hibbert.

2. A detailed Report on the state of the Communications and other writings in the repositories of the Society,—by John Dillon, Esq. Sec.

3. An account of some Sculptured Stones in the north of Scotland,—by Professor Stuart of Aberdeen.

4. The Catalogue of the Library of the Cathedral Church of Glasgow, dated A. D. 1432, with observations by John Dillon, Esq. Sec.

5. An account of some Antiquities in the island of Milo in Greece,—by Captain J. D. Boswall, R. N.

6. An account of an antient Axe of pure copper found at a great depth in Ratho Bog, county of Mid Lothian *,—by Dr Brewster.

7. An account of some Warlike Weapons from the remotest district of Nepaul,—by Dr Borthwick.

8. An account of some Monumental Tumuli at Largs,—by John Smith, Esq.

9. Observations on the Well-house Tower and North Loch of Edinburgh,—by James Skene, Esq.

10. Some Letters regarding the Porteous Mob,—communicated by Robert Johnston, Esq.

11. Account of an ancient Tomb lately discovered in Kincardineshire, supposed to be that of Malcolm I. of Scotland,—by Professor Stuart of Aberdeen.

12. Catalogue of the Manuscripts and Books belonging to the Franciscan Friars of Aberdeen,—by ditto.

13. Account of the ancient Tomb in the Abbey Church of Paisley called Queen Bleary's,—by the Reverend Dr Boog of Paisley.

14. Account of some Runic Inscriptions in the Isle of Man, and a Monument of that kind from Norway, now placed on the Castle Hill of Edinburgh,—by John Dillon, Esq. and the Rev. Dr Jamieson.

The Society received the following donations to their Museum:

Presented by *Godfrey Meynell, Esq.*—of Roman medals, Faustina, Aurelius, L. Verus, Scipio and Hannibal, Nero; a counter-marked medal; a ticket-medal for the games; two silver coins of Athens and Thurium, the latter plated; four Egyptian, of the Ptolemies; three Greek; Scotch coins of Alexander III. and John; a series of twenty-nine Popes medals, copper.

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* See this *Journal*, Vol. VI. p. 367.

Two Roman urns, and specimens of pottery from Little Chester, near Derby, the antient Derventio.

A Roman key of brass or copper.

A ring or fibula.

A Roman spoon and ligula, of which a plate and account is given in Dean Batteley's Antiquitates Rupinæ; part of a comb found near a skeleton at Little Chester.

A supposed druidical ear-ring and bead, found near a rocking-stone in the high Peak of Derby.

A celt or chip-axe, found at Aldborough, Yorkshire.

Presented by *John Graham Dalyell, Esq.* his Remarks on the Antiquities of the Episcopal See of Aberdeen.

Presented by A. Z. Description of the Isles of Orkney, 1743; the Death-Song of Laddbrokar, 1783; Martin's Description of the Western Isles, with Brand's Description of the Orkney Isles.—*N. B.* This anonymous benefactor has been in the habit for many years of enriching the collection of the Scotch Antiquaries with many valuable works, both antient and modern, connected with the history of the Orkney Islands and of Norway, to which many volumes of manuscript researches and drawings are added, forming a body of authorities and information on these subjects which is perhaps unequalled.

Presented by *George Chalmers, Esq.* the Ensign of King David I. of Scotland at the battle of the Standard A. D. 1133.

Presented by *Sir Thomas Brisbane*, a gold coin found in the field of Agincourt, in an excavation made at the place where many of the slain were buried, on which is the following legend: KAROLUS DEI GRATIA FRANCORUM REX X″P.C. vincit XP.C. regnat X″P.C. imperat.

Presented by *James Haig, Esq.* Theatrum Historicum Helvici, Edin. 1638.

Presented by *G. S. Menteith, Esq.* of Closeburn, a small Urn, of coarse earthen-ware, containing fragments of decayed bones, and four arrow heads of flint, nearly of a triangular form, and singularly gróoved and serrated.

Presented by *James Hotchkis, Esq.* a Silver Medal, having a lion sejant, with a sword in his dexter paw, and a fleur de lis in his sinister: Legend, IO *annis* METELLANUS LAUDERIÆ 60 *meo* (reverse) Consilio et annimis 1646.

Presented by *Henry Jardine, Esq.* the Plates of the engravings of the Scotch Regalia, and Seals of the Exchequer of Scotland.

Presented by the *Commissioners of Public Records,* vols. 7. & 8. of the Acts of the Parliaments of Scotland.

Presented by *Dr Duncan* his Life of Sir Joseph Banks, K. B.

Presented by *General Hutton*, a copy of the curious Portrait in fresco of King William the Lion, which belonged to the Monastery of the Trinity Friars of Aberdeen. The original is 4 feet high, by 2 feet 9 inches in breadth. The following lines have been added at the bottom: "Sir William, King of Scots, surnamed the Lyon, the first founder of the Trinity Friars of Aberdeen, where he had his chappell, the chief place of retirement for his devotions. He reigned 49 years, beginning 1165; dyed at Striveling 1214, and was buried at Aberbrothick."

Presented by *Henry Jardine, Esq.* copy of the Report on the Disinterment of King Robert the Bruce, with a morsel of the gold tissue which enveloped the body, and part of the ornaments of the ancient tomb, consisting of some small pillars of marble gilt, and a head of the king; likewise a cast of that monarch's skull, taken at the time of the disinterment. The coffin-plat of copper found in the grave, containing the device and name of Robert, was deposited in the Museum, by order of the Barons of Exchequer.

Presented by *Alexander Douglas, Esq.* 43 Copper coins.

Presented by *Captain J. D. Boswall,* R. N. an Egyptian coin of the Ptolemies.

Presented by *James Skene, Esq.* nine antient Scotch and English Coins, found in clearing out the rubbish of the Well-house Tower of Edinburgh; likewise part of an antient oaken Coffin found under a cairn in Aberdeenshire; a French Proclamation regarding the affairs of Scotland in the year 1745.

Presented by *James Maidment, Esq.* a reprint of an unique work preserved in the Advocate's Library, entitled, "Declaration of the Nobilitie to protect the King against the ennemies of his government and of true religion, 1582."

Presented by *Thomas Sivright, Esq.* the Head of a Roman Battle-axe.

Presented by *Sir Walter Scott, Bart.* 12 antient Coins found on the Castle Hill of Jedburgh.

Presented by *Sir George Stuart Mackenzie, Bart.* some remains found in the grave of a Bishop of Ross, lately opened at Fortrose.

Presented by —— *Pitcairne, Esq.* a reprint of the "Battayle of Flodden Felde, or Branstone Moor."

We are happy to observe, by the foregoing list of contributions, that the Museum of this Society is so much on the increase. The deficiency in public collections connected with the various pursuits of science and history, have long been a matter of regret to ourselves, and of surprise to strangers. That most important branch, natural history, of which, till within these very few years, no public collection whatever existed, is now in progress, and likely to become highly creditable to the scientific character of this city, as well as supplying an invaluable source of information to the student. We could wish that this collection of subjects of antiquity, of which a very respectable foundation is laid, were more generally an object of encouragement, so as to constitute a common receptacle for objects of this description; when collected together, they supply a very valuable record of antient manners and history, offering facilities to antiquarian research, which is quite unavailable while they remain scattered about, and neglected, in the hands of individuals, as well as inaccessible to persons engaged in such inquiries. We understand that the collection of Coins in the progress of formation by this Society, though as yet far from complete, is considerably advanced. The importance of antient coins, and the particular circumstances under which they may be found, are only valuable when collected into a series, as evidences of ancient history.

Where the national antiquities of a country are so much a subject of interest with every one in Scotland, we should expect that a Museum, particularly and exclusively devoted to the collection and preservation of such objects, and depending for its increase upon contributions alone, would attract more generally than seems to have been the case, the favour of those who may have it in their power to add to the collection. The Museum had no doubt for some years been allowed to fall into a state of neglect, as happened with the Natural History Collection of the College: this last has now risen into splendour from its ashes; and as the Museum of the Antiquarians has been recently arranged, and put upon such a footing, that due regard and advantage is taken of whatever is sent to the Collection, we hope soon to see it assume the important station it ought to hold as a public collection of Antiquities relative to Scotland.

ART. XXXIII.—SCIENTIFIC INTELLIGENCE.

I. NATURAL PHILOSOPHY.

ASTRONOMY.

1. *Great Comet of* 1811.—The great size and beauty of the comet of 1811, and the long period during which it continued visible, from 27th March 1811 to August 17. 1812, render it one of the most remarkable which has appeared in modern times. Several elements of its orbit have been published by different astronomers; but, with the view of taking into account all the smaller corrections, such as the precession, aberration, nutation, parallax, and the disturbing forces of the planets, M. F. Argelander of Konigsburg has recomputed its elements, and the details of his computation will occupy a separate work, which is now in the press. The basis of M. Argelander's calculations are the elliptic elements of M. Bessel; and his data are those obtained from Zach, Bessel, Pond, and Wisniewski. The following are the elements which he obtained:

		D.	Limits of Error.
Passage of Perihelion, M. Time at Paris, September,		12.261874	± 0.00109
Distance of Perihelion from Node, - -		65° 24′ 7″.891	± 3″.911
Longitude of Perihelion, - - - -		75 0 58.581	± 4.656
Longitude of Node - - - -		140 25 6.472	± 2.526
Inclination of Orbit, - -		106 57 33.833	± 2.250
Perihelion distance, - -		1.03540074	± 0.00001
xcentricity, - -		0.99515325	± 0.00005
riod, - -	Years,	3122,45	± 50.4

By taking into account the disturbing forces of the planets after the comet had ceased to become visible, M. Argelander obtained the following elements;

	D.	Limits of Error.
Passage of Perihelion, M. Time at Paris, September,	12.263804	± 0.00095
Distance of Perihelion from Node, - -	65° 24′ 10″.026	± 3.″597
Longitude of Perihelion, - - -	75 0 33.926	± 3.941
Longitude of Node, - - -	140 24 43.952	± 1.610
Inclination of Orbit, - - -	106 57 38.765	± 1.270
Perihelion distance, - - - -	1.03542283	± 0.0000083
Excentricity, - - -	0.99509330	± 0.000043
Period, - - - - Years,	3065.56	± 42.9

See Shumacher's *Astronomische Nachrichten*, No. 9. p. 119.

2. *Greenwich Observations on Venus and Jupiter, compared with the Tables of Lindenau and Bouvard.*—The Board of Longitude has just published a pamphlet, entitled, "The Longitudes and Latitudes of Venus and Jupiter, deduced from observations made at the Royal Observatory, compared with the Tables of Lindenau and Bouvard." There are 147 observations of *Venus*, one of which is rejected, and is not calculated for latitude, and one is exactly the same in longitude as the place calculated by Lindenau's Tables. The remaining 145 observations give the *mean* of Lindenau's Tables for longitude — 1″.8, and the *maximum* of error — 13.″7. In latitude, the *mean* error is — 3″.4, and the *maximum* — 14″.7. There are 66 observations of *Jupiter*, and they give the *mean* error of the same Tables for longitude + 13″.3, and the *maximum* error + 31″.5. For latitude, the *mean* error is + 5″, and the *maximum* + 10″.

The greatest errors in longitude are confined to 1820, since, from the observations of 1819, the *mean* error is 0″.3, and the *maximum* is ± 6.2; whereas, from the observations of 1820, the *mean* error is + 22″.8, and the *maximum* + 31.″5. Three longitudes of 1820 are calculated likewise by Bouvard's Tables of 1821, and by them the *mean* error comes out + 11″.1, and the *maximum* + 20″.0. It is remarkable that all the errors of both Tables are in excess for the longitude in 1820; and that of the 66 observations 64 give the error of the Tables in excess for the latitude.

3. *Longitudes deduced from the Solar Eclipse of 7th Sept.* 1820.—Professor Wurm of Stutgard has deduced the following longitudes in time from the eclipse of the 7th September 1820:

Madrid, -	24′ 8.9 W. of Paris.	Riga, -	1^h 27′ 8″ East.
Greenwich, (Pond),	9 20.3 West.	Nienstedten,	30 7.5
Amsterdam (Keyzer),	10 6.2 East.	Moskau,	2 21 20
Geneva, -	15 13.8	Bologna,	37 5.0
Zurich, - -	24 56.8	Bogenhausen,	37 39
Marpurg, -	25 43.6	Bergen, -	11 46.9
Genoa, - -	26 27.1	Speier, -	24 25.1
Hamburg (Rumker),	30 38.1	Darmstadt,	25 16.9
Marlia, near Lucca,	33 2.7	Cuxhaven,	25 31.6
Rome (Colleg. Rome),	40 34.3	Regensburg,	39 3.7
Copenhagen,	41 5.0	Fiume, -	48 21.6
Dresden (Math. Sal),	45 47.2		

Shumacher's *Astronomische Nachrichten*, No. 9. p. 131.

4. *Astronomical Positions of Places.*—M. Hansteen of Christiania has found, from numerous observations, that the longitude of Christiania, East of Paris, is 33′ 39″.

M. Knorre and Admiral Greig have determined the latitude of Nicolajef to be 46° 58′ 33″, by a repeating circle of Reichenbach.

Professor Waldeck of Abo has found the position of his observatory, which is the most northern in the world, to be 60° 26′ 58″ North Lat. and 1^h 19′ 48″ East of Paris. He expects, however, to be able to obtain a more accurate determination with the instruments which he is receiving from Munich.

Professor Struve of Dorpat makes the longitude of his observatory 1^h 37′ 35″ East of Paris, and its latitude 58° 22′ 47″. The latitude observed by a circle of Baumann's is correct within 2″; but there may be an uncertainty of a few seconds in the longitude.

5. *Comet of* 1821.—This comet was discovered at the Royal Observatory of Paris by M. Nicollet on the 21st January 1821, and on the same day by M. Pons in La Marlia. Dr Olbers found it on the 30th January; and it was observed at Slough by Mr J. F. W. Herschel on the 27th February. The following are its elements:

Passage of Perihelion, March 21. 1821,	9^h 33′ 7″ Mean Time at Paris.
Perihelion distance, - - -	0.091113
Longitude of Ascending Node, - -	48° 32′ 12″
Longitude of Perihelion, or Orbit, - -	239 18 37
Inclination of Orbit, - - -	74 10 53
Heliocentric Motion,	Retrograde.

The following are the elements of other astronomers:

	Prof. Encke of Seeberg.	Prof. Nicolai of Manheim.	H. Von Staudt of Gottingen.
Passage of Perihelion,	Mar. 21. 405	Mar. 21. 6016	Mar. 21. 6026
Longitude of Perihelion,	239° 20′ 45″	239° 34′ 5″	239° 36′ 0″
Long. Perihelion distance,	8.95966	8.96466	8.9641627
Long. of Node, -	48° 34′ 37″	48° 43′ 34″	48° 45′ 44″
Inclination of Orbit,	74 5 0	73 23 15	73 16 33

The following interesting observations on the comet were made by Mr J. F. W. Herschel with the 7 and 10 feet reflector at Slough. With neither of these could he discover any central star-like point, such as he had distinctly seen in the comet of 1819. It seemed a mere misty mass. The tail was about 2½° long, and seemed to be somewhat less bright along its axis. The head seemed rather obscure, and appeared to have lateral portions of light, which seemed to go off at a greater angle than the tail.—See *Memoirs of the Astronomical Society*, vol. i. p. 154. 156, 157.

6. *Supposed Volcanoes in the Moon.*—The luminous appearance in the moon, which Captain Kater and Mr Dunlop observed on the 5th February 1821, and which Captain Kater considered as a lunar volcano, was observed by Dr Olbers, who thinks that there are no volcanoes in the moon, and that this phenomenon is capable of another explanation. It was situated, he observes, either in or near the spot marked Aristarchus, which is always enlightened by the earth, or the dark portion of the moon when three or four days old, and is distinguishable from all the other spots in the moon by its brightness. The luminous appearance, however, on the 5th of February was entirely different from the usual appearance of Aristarchus, and appeared with a five feet achromatic telescope like a star of the 6th magnitude.

Professor Struve of Dorpat observed the very same phenomenon on the 27th January 1822. He describes it as a point near Aristarchus, resembling a star of the 8th magnitude, and is of the same opinion with Dr Olbers that it is not a volcano.

The same phenomenon was seen in Aristarchus on the 4th and 5th May 1821 by Mr Ward at Tamworth, and by Mr Bailey. Hevelius describes the spot called Aristarchus under the name of *Mons Porphyrites*, and describes it as *aut ex rupe rubra, aut sabulo, sive terra rubicunda constare, aut prorsus* *ardere, sive perpetuo igne exundare.* Mr Ward supposes that its colour must not have changed, as it is singularly white when illuminated by the sun, and preserves its predominant whiteness when the other parts of the moon are yellow, or faintly red.— See *Mem. Astron. Soc.* vol. i. p. 158. 161.; and Shumacher's *Astron. Nach.* No. 9 p. 138.

7. *Perturbations of the New Planets.*—The celebrated M. Gauss of Gottingen has determined, in an exact manner, the masses of those planets which exert a sensible influence on the four new planets. He shews clearly, that the mass of Jupiter given by Laplace is wrong by more than a tenth part. The perturbations of Pallas produced by Jupiter amount to several degrees, and afford a very certain means of determining the mass of the latter.

8. *Right Ascensions and Declinations of Juno.*—The following are the right ascensions and declinations of Juno, as calculated by Professor Nicolai, for noon at Manheim, *33′ 52″* E. of Greenwich:

	R. Ascen. in Time.	Declin. North.	Log. Dist. from Earth.
1822, Oct. 21.	7h 40′ 16″	6° 0	.2434
Nov. 2.	7 53 9	4 21	.2161
Dec. 4.	8 7 41	0 55	.1434
1823, Jan. 1.	7 54 22	0 44	.1033
☍ —— 17.	7 40 13	2 17	.1048
Feb. 2.	7 27 39	4 42	.1277
Mar. 2.	7 21 5	9 16	.2040
April 3.	7 40 12	12 50	.3076
—— 7.	7 44 7	13 7	.3200

Bode's *Jahrb.* 1822.

9. *Right Ascensions and Declinations of Vesta.*—The following are the right ascensions and declinations of Vesta, calculated by Prof. Encke for Seeberg, at noon, 20′ 30″ E. of Greenwich:

	R. Ascen.	Declin. South.	Log. Dist. from Earth.
1822, April 1.	266° 50′	17° 8′	.2226
May 1.	271 3	17 14	.1338
June 5.	266 56	18 25	.0619
☍ —— 15.	264 26	19 1	.0574
July 5.	259 43	20 20	.1504
Aug. 4.	257 31	22 26	.0755
—— 27.	261 16	24 3	.2267

Bode's *Jahrb.* 1822.

10. *Declination and Meridional Passage of Ceres.*—The following are the declinations and times of passing the meridian of *Ceres* at Berlin, 55′ 29″ East of Greenwich :

	Declination.	Passage of Meridian.
1822, Jan. 1.	26° 1′	0h 19m P. M.
Feb. 1.	24 54	11 1 A. M.
Mar. 1.	23 12	9 58
Apr. 1.	21 8	1 50
May 1.	19 28	7 38
June 1.	18 56	6 4
July 1.	20 6	4 13
Aug. 1.	23 17	2 2
Sept. 1.	26 30	11 37 P. M.
Oct. 1.	27 8	9 29
Nov. 1.	25 15	7 34
Dec. 1.	21 48	5 49

Bode's *Jahrb.* 1822.

1. *New Comet of* 1822.—On the 14th May, M. Pons of the Observatory of La Marlia, near Lucca, discovered a comet near the horizon in the north-west, at 9h 15′. It was then in Auriga. It resembles a star of the 4th magnitude, and is distinctly visible to the naked eye.

12. *Visibility of the Planet Venus* *.—This planet passed the point of its inferior conjunction with the sun, when its dark hemisphere is completely turned towards the earth, on the 10th current, at half past one o'clock in the morning; and, having now arrived to the westward of the sun, has, of course, disappeared from our evening sky. Its crescent was distinctly seen on the afternoon of Thursday the 7th March, only two days and twelve hours prior to the moment of conjunction, by Mr T. Dick of this city, who has made a variety of observations on this planet, in the day time. Cloudy weather prevented observations on the two following days, otherwise it might have been perceived in a position much nearer to the sun. On Monday last, the 11th current, at 30 minutes past 12 noon, being only 35 hours after the planet had passed the point of conjunction, its crescent on the opposite hemisphere was again distinctly perceived, by the same observer, by means of an Equatorial Telescope, magnifying about seventy times. It appeared extremely slender but distinct, and well defined, and apparently of a lar-

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* See this *Journal*, Vol. III. p. 191.

ger curve than that of the lunar crescent, when the moon is about two days old. The difference of longitude between the Sun and Venus at that time was about 2° 19′. This is the nearest position to the point of conjunction in which Venus has been seen in any former recorded observation; and, from the degrees of distinctness with which the planet appeared, there is little doubt that its crescent would have been perceptible twelve hours before, had it been then above our horizon. From this observation the following conclusion is deduced, viz. that "from the time that this planet ceases to be visible prior to its inferior conjunction, on account of the smallness of its crescent, and its proximity to the sun, to the moment when it may again be perceived in the day-time by an Equatorial Telescope, there elapses a period of only 2 days and 22 hours;" or, in other words, Venus can never be hid from our view about the time of her *inferior* conjunction, for a longer period than 70 hours. From observations formerly made on this planet about the time of its *superior* conjunction, by the same observer, it appears, that Venus can be hid only 6 days 8½ hours *before*, and the same time after that conjunction; and, consequently, it will follow, that, during the space of 582 days, the time she takes in moving from one conjunction to a like conjunction again, she may be seen by means of an Equatorial Telescope, every clear day, without interruption, except during a period of 15 days and 15 hours.—*Perth*, 13*th March* 1822.

OPTICS.

13. *Laws of Polarisation in rectangular Plates of Glass.*—If we call ∂, ∂', the distances of any point from the lines of no polarisation in rectangular plates of glass, crystallised either transiently or permanently by heat, the tint t at that point will be $t = T \partial \partial'$, T being the maximum tint between the black lines. In like manner, $v^2 = V^2 + a \partial \partial'$ will express the square of the velocity of the extraordinary ray, V^2 being that of the ordinary one, and a a co-efficient, varying with the plate of glass.—D. B.

14. *Frauenhofer's Experiments on the Prismatic Spectrum.*—M. Frauenhofer of Benedictbaiern, near Munich, who has been long celebrated for the excellence of his achromatic telescopes, has made, a considerable time ago, some important and curious discoveries respecting the prismatic spectrum. He found, that

the spectrum when formed by all bodies, whether solid or fluid, is crossed with above 500 dark lines of various magnitudes, parallel to one another, and perpendicular to the length of the spectrum. He found also, that these lines had fixed positions, and by measuring the distance between the most prominent in different spectra, he obtained precise measures of the action of the prism on the rays that formed the corresponding coloured spaces. The spectra produced by the two polarised pencils of Iceland spar, exhibited the same lines. In order to see these lines, prisms perfectly free of veins must be used, and all extraneous light carefully excluded. The experiments of M. Frauenhofer were first published in the *Memoirs of the Academy of Bavaria*, for 1815.

15. *Frauenhofer's Experiments on the illuminating Power of the Prismatic Rays.*—By means of an ingenious photometer, M. Frauenhofer measured, with great care, the illuminating power of the different coloured spaces, and obtained results very different from those usually given: On each side of the yellow space, the light varied with very great rapidity, as appears from the following measures:

	Intensity of Light.
At the 22d degree [*] of the Red, - -	0.032
At the 34th degree of the Red, - -	0.094
At the 22d degree of the Orange, - -	0.64
At the 10th degree of the Yellow, - -	1.000
At the 42d degree of the Yellow, - -	0.48
At the 2d degree of the Blue, - -	0.17
At the 16th degree of the Indigo, - -	0.031
At the 43d degree of the Violet, - -	0.0056

The measures here given have no relation to the colours opposite to them, as the colours are mentioned merely to point out the position in Newton's spectrum, corresponding to the position in Frauenhofer's spectrum, where the intensity of illumination was measured. The colours in Frauenhofer's spectrum, indeed, do not correspond with those of Newton.

16. *Frauenhofer's Experiments on the Inflexion of Light.*—M. Frauenhofer has recently printed a very interesting disser-

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[*] The whole length of the spectrum is here supposed to be divided into 360°; the *Red* occupying 45°; the *Orange* 27°; the *Yellow* 48°; the *Green* 60°; the *Blue* 60°; the *Indigo* 40°; and the *Violet* 80°.

tation on the Inflexion of Light, and has discovered many curious phenomena which have escaped the notice of all preceding philosophers. His experiments, indeed, were made on a scale, and with an apparatus much larger, and more accurate, than had hitherto been employed. The repetition of his experiments will be a matter of some difficulty, as it requires the best instruments, and the nicest precautions to develope the very remarkable spectra which he discovered to be produced by the inflexion of light passing between wires, or transmitted through one or more apertures arranged in different forms.

17. *Phosphorescence of the Sulphates of Quinina and Cinchonina.*—M. Callaude has found, that this substance becomes luminous when exposed to a gentle heat. M. Pelletier has likewise discovered, that sulphate of cinchonina, both alone and when mixed with the sulphate of quinina, become luminous when exposed to the steam of boiling water; but that neither quinina nor cinchonina by themselves, nor their acetates, possess the property of being phosphorescent by heat. *Journ. de Pharmacie,* Sept. 1821.

18. *Singular effect of Heat on the Spinelle of Ceylon and Aker.*—M. Berzelius, in his very interesting and valuable work on the Blowpipe, just translated by Mr Children, has remarked, that the spinelle becomes brown by heat, and then blackens and becomes opaque as the heat increases. It then resumes its colour in cooling, in the following manner: When seen by daylight, its colour, by transmitted light, is a fine chrome green, then it becomes colourless, and finally resumes its red colour. This result has such a general resemblance to that obtained from the corundum ruby*, that the two minerals used may be supposed to have been the same. This, however, was not the case, as the ruby employed by Dr Brewster was a doubly refracting crystal with one axis. M. Berzelius does not seem to have observed any peculiarity in the action of heat upon the corundum ruby. See Berzelius, *De L'Emploi du Chalumeau*, p. 289, 252.

ACOUSTICS

19. *On Sounds e* *ogen Gas.*—As the intensity of sound is diminis fication of the medium in

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* See this *Journal*, Vo

which it is produced, it might have been expected that the sound in hydrogen gas would be feebler than when produced in atmospheric air in similar circumstances. Mr Leslie, however, has found the difference to be actually much greater. Having placed within the receiver of an air-pump a small piece of clock-work, by which a bell was struck every half minute the air was rarified, and after the reacation had been carried the length of 100 times, hydrogen-gas was introduced. The sound, however, so far from being augmented, was at least as feeble as in atmospheric air of that extreme rarity, and decidedly much feebler than when formed in air of its own density, or rarified ten times. Mr Leslie likewise observed the very curious fact, that the mixture of hydrogen gas with atmospheric air, has a predominant influence in blunting or stifling sound. When one half of the volume of atmospheric air is extracted, and hydrogen gas admitted to fill up the vacant space, the sound will now become scarcely audible; an effect which he ascribes to a want of intimate combination between the two gases, which causes the pulsatory impressions to be dissipated before the sound is originally formed. *Mem. Cambridge Phil. Soc.*, vol. i. p. 267.

ELECTRICITY.

20. *On the ordinary and extraordinary Electricity of Minerals.*—The effect of heat in developing electricity in certain minerals, has been long known. M. Haüy, however, has discovered, that the electricity which he had formerly observed in minerals, instead of vanishing abruptly at the ordinary temperature at which it seemed to disappear, had only reached the node through which it passed to an opposite state, by a farther reduction of temperature. With *oxide of zinc* and *tourmaline* he invariably found, that the opposite electricity could be developed by cold, so that the pole which possessed *vitreous* electricity when it was *hot*, developed resinous *electricity* when it was *cold*. M. Haüy calls the electricity produced by heat *ordinary*, and that which is produced by cold *extraordinary*,—terms borrowed from the phenomena of double refraction, but quite inapplicable to the present case.

METEOROLOGY.

21. *Formation of Clouds.*—The following observations on the formation of clouds, we owe to W. C. Trevelyan, Esq. who

spent the greater part of last summer and autumn in the Faroe Isles: "We frequently saw clouds, when precipitated from a clear atmosphere by a mountain (as described in Dr MacCulloch's *Western Isles*, vol. i. p. 475.) undergo a perpetual increase at that end from which the wind came, and a corresponding decrease at the other, sometimes invisibly, and at other times in the form of small clouds. I also occasionally observed that those ends of the mountains towards the point from which the wind came, were capped with *strati*, and the other parts with *cumuli*. Sometimes a thin cloud is gradually formed on the side of a mountain, and, after a few minutes, disappears as gradually: the whole process, from its first appearance to its final dissolution, being performed in not more than twenty or thirty minutes."

22. *Aurora Borealis in Faroe and Shetland.*—Mr Trevelyan observed, that the aurora borealis in Faroe and Shetland was often seen very low, not more than 40 or 50 feet above the sea, and he learned that in both countries it is frequently heard. In Faroe, Mr Trevelyan met one person who stated, that when the colour of the aurora borealis is dark red, and extends from west to east with violent motion, he had experienced a smell similar to that which is perceived when an electric machine is in action.

23. *Great Fall of the Barometer on the 25th December* 1821. —As there is reason to believe, that the extraordinary fall of the barometer on the 25th December 1821 was connected with the volcanic eruption of Eyafjeld Jokkul, Iceland, it becomes inteteresting to collect the height of the barometer on that day in different parts of Europe ;

	In. lines.	
Naes in Iceland, near Reikavig,	28.49 Eng.	Dec. 26.
Cambridge,	28.00 do.	Dec. 25.
Hanover,	28.34 do.	Dec. 25. 1 P. M.
Altona,	28.31 do.	Dec. 25. 2½^{h}
Udino,	25.5.1 French.	Dec. 25. Morning.
Bremen,	26.676	Dec. 25. 6^{h} Stormy.
St Bernard,	19.10	Dec. 24. after Midn.
Fougeres,	26.2.4	Dec. 24. 9^{h}
Brest,	26.3	Dec. 24.
Lyons.	25.9	Dec. 24. 9^{h}
Jena,	26.3	Dec. 24, 25.
Treves,	26.6	Dec. 25. 5^{h} A. M.
Augsburg,	25.6.1	Dec. 25. 3¾ A. M.
Leipsic,	26.68	Dec. 24. 25.

24. *Meteors and Lightning accompanying the Fall of the Barometer on the 25th December* 1821.—The extraordinary fall of the barometer above described, (See also this *Journal*, Vol. VI p. 383.) was accompanied with igneous meteors and lightning in many places. At *Bamberg* and at *Frankfort*, an igneous meteor was seen on the 24th December, at 7ʰ P. M. It had the shape and apparent size of the full moon; moved in a north-easterly direction, and disappeared with an explosion as loud as that of a cannon. At *Vienne* in France, the heavens appeared all on fire, and the thunder and lightning continued nearly an hour. At *Clermond-Ferrand*, on the 24th, at 9ʰ P. M. there was a dreadful storm of thunder, lightning, and rain. Near *Dijon*, the thunder was so alarming in the rural commune of the Cote d'Or, that the inhabitants were driven from the church; and about 10ʰ P. M. of the 24th, the whole horizon seemed on fire with the lightning.—*Bibl. Univers.* Jan. 1822.

25. *Storm at Genoa on the 24th December* 1821.—The following extract of a letter from Genoa is given by Mr Hailstone in the Cambridge Memoirs: " The inhabitants of Genoa have often witnessed ravages occasioned by tempests, but not one so dreadful and prolonged as that which we experienced during the night of the 24th instant. It will ever be memorable in the annals of our state. During several days previously, the air was charged with thick vapours, which vented themselves in torrents of rain: the wind was S.E.; on the 24th, at 6ʰ P. M. it settled in the south, and blew with intense violence; at ten o'clock it had reached its utmost force. The sea rose progressively. At 11ʰ the vehement conflicts betwixt the two elements had the full character of a hurricane, and, in the language of the country, was a *terremoto di mare*." Other accounts from Genoa state, that the loss of merchant ships was incalculable, the crew and cargo of some of them having perished in the very harbour. This storm seems to have been limited to the south by the parallels of Tuscany and Romagna.

26. *Devastations in the Canton of Appenzel.*—The damage done by the dreadful storm of the 24th December seems to have been enormous in the Canton of Appenzell. The oldest inhabitant never witnessed such a scene of devastation and terror.

Almost every house in the commune of Gaiss has experienced some damage. In some districts, the roofs of the houses have been carried away, a great number of barns, destroyed, and immense portions of the forests laid waste by the storm. Similar calamities were inflicted on the Rhinthal.—*Bibl. Univers.* Jan. 1822.

27. *Inundations accompanying the fall of the Barometer.*—In almost all those places where the hurricane was experienced, deluges of rain accompanied it. In Piedmont, the bridges, the roads, and many houses, have been carried away by the floods. The Royal Road from Tortona to Plaisance was submerged, and also the road from Turin to Novi. The roads to the east and west of Genoa have suffered great injury. Savona was surrounded with the inundation, and all communication cut off with Genoa and Nice. The province of St Nevero has been almost entirely covered by the sea, and its olive trees destroyed. At Trieste, all the low streets were under water. At Venice, they were obliged to cross the Place of St Mark in boats; and at Leghorn and Genoa the sea penetrated into several magazines, and occasioned great loss.—*Bibl. Univers.* Jan. 1822.

28. *Earthquake accompanying the Fall of the Barometer.*—On the 25th December, about half past eight o'clock in the evening, a slight shock of an earthquake was felt at Mayence.

II. CHEMISTRY.

29. *Nitrate of Soda discovered in the district of Tarapaca in Peru.*—This discovery promises to be of great use in the chemical arts, particularly in the manufacture of nitric acid and saltpetre. More than 60,000 quintals of this salt, purified by dissolution and crystallisation, have been sent into the ports of Conception in Chili, and Iquiqui, a port in the south of Peru, from both of which it is distant only about three days journey. This salt forms a bed several feet thick, which in some places appears at the surface, and which occupies an extent of more than 40 leagues. It is sometimes in a state of efflorescence, and sometimes crystallised, but more frequently mixed with clay and sand. Its taste is fresh and bitter. It is deliquescent, and burns like nitre. It contains a little sulphate of soda. According-

ng to Haüy, it crystallises in the form of a rhombohedron, in which the ratio of the diagonals of the transverse section are as 2 to 4. The obtuse angle of this section, is 102° 38′, and the obtuse angle of the principal section 110° 38′.—Note by M. Mariano de Rivero in the *Annales des Mines*, 1821, tom. vi. p. 596.

30. *Analyses of different kinds of Mica.*—M. Peschier of Geneva has analysed several different kinds of mica, and has discovered *Titanium* in this mineral. The following are his results, with three kinds of mica, with one axis of double refraction:

	Green Mica of Vesuvius.	Black Mica of Vesuvius.	Foliated Black Mica of Siberia.
Silica, -	45.7	42.0	35.5
Alumina, -	31.7	8.35	11.25
Magnesia, -	0.95		
Lime, -	10.75	15.7	
Iron, -	6.8	8.35	16.0
Titanium, -	0.1	15.0	30.0
Soda, - -		8.5	1.7
Lithion, -		2.5	
Potash, -			6.1

In the first and third of these varieties, a trace of manganese was found. M. Peschier has also discovered titanium in the white mica of Siberia, and in the mica of Massachussets.—*Journ. de Phys.* xciii. p. 241.

31. *Analysis of Alum.*—The following are the results of Dr Thomson's analysis of alum:

4 Atoms Sulphuric Acid, -	20.0	or	32.8542
3 Atoms Alumina, -	6.75		11.0882
1 Atom Potash, -	6		9.8562
25 Atoms Water, -	28.125		46.2012

otherwise,

3 Atoms Sulphate of Alumina,	21.75	or	35.72885
1 Atom Sulphate of Potassa,	11.00		18.06975
25 Atoms Water, - -	28.125		46.20123

Dr Thomson conceives that there is no evidence of there being any bisulphate of potash in alum.—*Annals of Philosophy*, vol. iii. p. 168.

32. *On the Heat developed in solutions Crystallised by Exposure to the Air.*—When 51 parts of sulphate of soda and 49 parts of water are placed in a phial, and the stopper removed in

a temperature under 50°, the crystallisation of the mass takes place, and there is a rise of temperature equal to 24° of Fahrenheit. When the carbonate of soda is used, small crystals or stars are abundantly precipitated, and the temperature rises 14°. From a series of experiments made in this way, Dr Thomson concludes, that the latent heat of the water of crystallisation is in both cases the cause of the increase of temperature.—*Ann. of Phil.* iii. p. 169.

33. *On the Existence of Mercury in Sea-Water.*—M. Proust has remarked, as M. Hilaire Rouelle did before him, that marine salt contains mercury. M. Proust has found mercury in every kind of muriatic acid that he has tried, and also in rock-salt. He suggests to navigators a method of ascertaining the existence of mercury in sea-water, by attaching a plate of gold, of two or three inches surface, to some part of the ship, so as to be constantly plunged in the water. Half an ounce of gold laminated, he conceives would be sufficient for the purpose of ascertaining if it is amalgamated after a long voyage. M. Proust offers to be at the expence of the experiment, and desires any person willing to make it to apply to M. Lucas, agent to the Institute.—*Mem. de Museum*, vii. p. 479.

III. NATURAL HISTORY.

MINERALOGY.

34. *Merian's Account of Alpine Limestone.*—Professor Merian of Basle, in a little work entitled "Ubersicht der Beschaffenheit der Gebirgsbildungen in den Umgebungen von Basel, 1821," we are informed, arranges the Jura limestone, oolite, shell limestone, and alpine limestone, as members of the same series; and in the Basle district, these limestones rest upon the second or new red sandstone formation. This arrangement agrees with that previously adopted in this country. Raumer, in a geological tract published some years ago, stated, as a very probable conjecture, "the identity of the shell limestone, oolite, and alpine limestone."

35. *Oxygen in* .—Sir H. Davy has ascertained by experiment, th tained in vesicular cavities in

rock-crystal is impregnated with oxygen, like that observed in some springs, and that the superincumbent air is azote.

BOTANY.

36. *Distribution of Vegetables in Madeira.*—Kuhl, during his stay in Madeira, was enabled to obtain a general conception of the distribution of its vegetables; and the following is an abstract of the different vegetable zones observed in ascending from the sea-shore to the higher parts of the island.—1. *Region of the Cacti.* This region extends from the sea-shore to the height of 630 feet. Here the Cactus and Ficus indica grow on the rugged rocks, and the vine, sugar cane, arum, and other fruits of southern climes, in the fields. This region is also abundant in wild plants, &c. of various descriptions.—2. *Region of the Vine.* The cultivation of the vine commences on the sea-shore, and the cacti accompany it to the height of 630 feet. The vine ascends to an elevation of 2030 feet; but above this elevation the vine no longer bears ripe fruit. The arum, sugar-cane, mulberry, potato, and allium, are cultivated; but neither cactus nor banana are here met with. Agriculture is more extensively followed in this than in any other region.—3. *Region of the Chesnut-tree.* The lower edge of this region is 2300 feet above the sea, and is distinguished by its lofty trees, which are found to the height of 2950 feet. At a greater elevation, they are small, and no longer bear fruit.—4. *Region of the Genista.* In this region, whose upper limit is 3920 feet high, whole tracts are covered with genistæ. —5. *Region of Heaths.* This zone extends to the summit of Pico-Ruwa, which forms the highest peak in the island, and which is reckoned 5300 feet above the level of the sea, and, as its name implies, abounds in heaths.

ZOOLOGY.

37. *Dr Æscholz on Skeletons of Insects.*—Dr Æscholz has lately described what he considers as an internal skeleton, which he detected in several tribes of the class Insecta; and the same naturalist has published some new views in regard to the structure of the heart, particularly of the right ventricle.

38. *M. Kuhl's Zoological Observations.*—From the observations of M. Kuhl, it appears that the bones of the Cape Petrel (*Pro-*

[illegible] no air cells, but are compact, al[illegible] found far at sea, and possess considerable [illegible] further observed that they swam little, and [illegible] their heart was not so triangular and de[illegible] the *Anas nigra*, and other birds that dive [illegible] long under the water. Kuhl has discovered [illegible] generation of the Arethusa (*Physalia*), and in [illegible] authors have described as tentaculæ; and the [illegible] naturalist has confirmed, by actual dissection, all [illegible] had conjectured in regard to the structure of the [illegible] and Porpita. The *Mammaria adspersa* of Tilesius, [illegible] finds to be the ovum of an asterias, or some other sea [illegible] Kuhl found vast abundance of crabs of the Pagura [illegible] in the Indian Islands. These paguras live on the land and avoid the sea, as was observed by Bosc of another species which lives in America: they abound not only on the heath, but also wander into the thickest woods, and, what is still more remarkable, climb the stems of the cocoa-nut tree and the arborescent Rauwolfia, in search of the eggs and young of the sula.

39. *Toad found alive in the Centre of a Stone.*—A specimen of a toad, which was taken alive from the centre of a mass of solid stone, has been sent to the College Museum of Edinburgh by Lord Duncan.

40. *Discovery of Bones of the Megatherium.*—The Batavian naturalists Spix and Martins, during their residence in Brazil, found in limestone caves bones of the Megatherium.

41. *Bones of the Mammoth.*—Several of the large bones of the mammoth have been lately discovered in the province of Groningen, and deposited in the public museum.

42. *Bones of Quadrupeds.*—Another fissure or cave, containing bones of quadrupeds, has been discovered in the limestone of Yorkshire.

43. *Cave near Sundwich.*—A cave, near Sundwich in Westphalia, 1500 yards in extent, has been found to contain bones and skeletons of ar[illegible] species of *bear*.

IV. GENERAL SCIENCE.

44. *Crystallisation of Water in Regular Rhomboids.*—On the 3d of January 1821, the late Dr Clarke observed at Cambridge regular crystals of ice, many of which were more than an inch long. He exhibited them to several members of the University, and frequently in their presence measured the angles of the rhombic crystals, with the goniometer of Carangeau, which he found to be 120° and 60°. After a thaw took place, the crystals preserved in melting the same inclination of their planes. The hexaedral prism of ice, therefore, which we have described in a former volume (Vol. II. p. 80.), as found in the subterranean glacier of Fondeurle, was a secondary form of the crystal. The above very interesting results confirm the anticipation of Dr Brewster, who, from the optical structure of Ice, referred it to the rhomboidal or pyramidal systems of Professor Mohs *. A full account of Dr Clarke's observations will be found in the *Memoirs of the Cambridge Philosophical Society*, vol. i. part ii. p. 209.

45. *Oil for Watch and Clock Work.*—Good oil has long been a desideratum among watchmakers. Colonel Beaufoy remarks, that if olive oil be exposed to the rays of the sun for a considerable length of time, it becomes colourless, limpid, free from mucilage, and not easily congealable. He exposed two eight-ounce phials, nearly filled with this oil, to the solar beams for one or two years, and found this effect produced. The bottles should be opened occasionally to allow the gas to escape, or the cork may be taken out.—The following process by Chevreul has been recommended for freeing oil for watch-work from all acid and mucilage. Put into a matrass or glass-flask, a portion of any fine oil, with seven or eight times its weight of alcohol, and heat the mixture almost to boiling, decant the clear upper stratum of fluid, and suffer it to cool; a solid portion of fatty matter separates, which is to be removed, and then the alcoholic solution evaporated in a retort or basin, until reduced to one-fifth its bulk. The elaine or fluid part of the oil will be deposited. It should be colourless and tasteless, almost free from smell,

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* See *Wernerian Memoirs*, vol. iii, p. 348.

without action on infusion of litmus, having the consistence of white olive oil, and not easily congealable.

46. *Remarkable Formation of Ice.*—While examining some subterraneous excavations in a bed of lava, near Niedermendig, M. Pictet observed, in some places, water falling drop by drop on the floor, or against the sides of the cavern. *Whenever this happened, there was beneath, a mass of ice of a certain thickness,* although the temperature of the air never exceeded 39°.8, and at no time descended to 32°.—*Mem. de la Soc. Phys. de Geneve.*

47. *Size and Shape of the Globules of Blood in different Animals.*—A number of very interesting results have recently been obtained by J. L. Prevost, M. D. and J. A. Dumas, respecting the form of the globules of blood of different animals, and the effects of transfusing the blood of one animal into another. The following are their measures of the diameters of the globules:

Man, Dog, Rabit, Pig, Hedgehog, Guinea Pig, Muscarden, - - -	$\frac{1}{3750}$ of an English inch.
Ass, - - - -	$\frac{1}{4173}$
Cat, Grey Mouse, White Mouse, - -	$\frac{1}{4273}$
Sheep, Horse, Mule, Ox, - - -	$\frac{1}{5000}$
Chamois, Stag, - - - - -	$\frac{1}{5450}$
She-Goat, - - - - - -	$\frac{1}{7200}$

But while the globules of blood in different animals vary in size, they vary also in form. In the mammalia they are all spherical, while in birds they are elliptical, and vary only in the lengths of their greater axes. They are likewise elliptical in all cold-blooded animals. They found also, that the colourless globule which exists in the centre of the particles of blood, has the constant diameter of $\frac{1}{7500}$th of an inch in all animals, and whatever be the form of the globule which contains it.—In their experiments on the transfusion of blood, they obtained many interesting results. When animals were bled till they fainted, they died when they were left alone, or when water or serum of blood, at the temperature of 100 Fahr. was injected into their veins. If, on the contrary, the blood of an animal of the same species was injecte [illegible] n of the blood thrown in, reanimated the exhaust [illegible] when it had received as much as it lost, it began t [illegible] to take food, and was finally restored to perfect health. When the injected blood was from an animal of a different species, but whose globules had the same form, though a different size, the animal was only partially relieved, and could seldom be kept alive for more than six days, the animal heat diminishing with remarkable rapidity. When the blood of an animal with spherical globules, is injected into a bird, it usually dies under the most violent nervous affections, as if under the influence of the most intense poison; and this takes place even when only a small quantity of blood has been lost. In a great number of cases, cats and rabbits were restored for some days by the injection of the blood of cows and sheep, even when the injection of the blood was not made till twelve or even twenty-four hours after the blood was extracted from the latter. The blood was kept in a fluid state in a cool place, either by taking away a certain quantity of fibrine, or adding 1000th part of caustic soda. When the blood of the sheep was injected into ducks, they died after rapid and strong convulsions.—*Bibl. Univers.*

48. *Subnitrate of Bismuth used in Intermittent Fevers.*—This medicine has been prescribed in agues for several years by Dr Henkesew of Hildesheim. He exhibits it in doses of four grains, with a few grains of sugar, every two hours, and considers it as a powerful febrifuge and antispasmodic.—*Quart. Journ.* No. 25. p. 233.

49. *Preservation of Grain in Granaries of Cast-Iron.*—In order to preserve grain for any length of time, from those insects which habitually devour it, and which cannot exist in air hygrometrically dry, M. Clement suggested the propriety of constructing granaries of cast-iron, into which no air should enter till it has passed through a body of unslaked lime. He proposed also some contrivances for allowing the expanded air to escape, and for inspecting the grain when necessary. The saving of manual labour in turning over the grain is one of the advantages of this plan. Various useful suggestions relative to this plan will be found in the *Quarterly Journal*, No. xxv. p. 164.

50. *Eruption of Mount Vesuvius.*—On the 13th of February, Mount Vesuvius indicated the approach of an eruption by violent detonations, which were renewed on the 16th and 17th.

On the 18th a thick smoke was discharged. On the 19th showers of cinders and fragments of inflamed lava were thrown out. On the 20th the lava was seen boiling in the crater, and threatening to burst over its sides; and on the 21st the lava forced its way into the southern part of the mountain, by a new opening, from which it flowed abundantly, at the rate of six feet per minute towards the hermitage of St Salvador. On the 24th, the violence of the eruption was redoubled, and in the evening Vesuvius presented to the inhabitants of Naples the superb spectacle of a river of fire, rolling amidst clouds of smoke along the flanks of the mountain. As the lava flowed through lands already burned, and entirely desert, no injury has yet been done by this eruption.

51. *Encroachment of the Sea on the Coast of America.*—At Cape May, where the River Delaware falls into the Atlantic, the following curious observations are inscribed on the wall of a house:

Distance of the House from the Sea.

	Feet.		Feet.		Feet.
1804,	334	1809,	267	1817,	214
1806,	324	1811,	259	1818,	204
1807,	294	1812,	254	1819,	188
1808,	273	1816,	225	1810,	180

New Monthly Mag.

52. *Memoirs of the Physical and Natural History Society of Geneva.*—The Physical and Natural History Society of Geneva was founded in 1790, by a number of those distinguished individuals at Geneva who devoted themselves to the pursuits of science. The principal papers read at their meetings, found a place in the Philosophical Transactions, in the Memoirs of the Institute, or in those of the Academies of Berlin and Turin; while those which were of a less elaborate nature were transmitted to the popular scientific journals of the day. In consequence, however, of the deliverance of Geneva from a foreign yoke, all the public institutions received a vigorous impulse; and the Physical Society, strengthened with an accession of members, as well as of zeal, found itself in a sit[illegible] print its own Memoirs. They have accordingly publis[illegible]art of the First Volume, and we understand tha[illegible]t is nearly ready for publication.

The following is a list of the articles in the part already published, which is adorned with the names and labours of Pictet, Prevost, Decandolle, Saussure, and Jurine.

Mémoire sur quelque particularités, de l'oeil du Thon (*Scamber Thynnus*, Linn.), *et d'autres poissons*, par L. Jurine.
Note sur les dents et la mastication des poissons, appellés Cyprins, par le même, - - - - - 18
De l'effet du mouvement d'un plan réfringent sur la refraction, par Mr. P. Prevost, - - - - - 25
Observations sur les rapports qui existent entre les axes de double refraction et les formes des cristaux, par Mr. F. Soret, - - - - - - - 33
Note sur le mica, par le même, - - - 89
Mémoire sur divers instrumens de physique et de météorologie, par Mr. P. Huber, - - - - 93
Mémoire sur la chute des feuilles, par Mr. P. Vaucher, 120
Notice sur la contrée basaltique des départmens de Rhin et Moselle, et de la Sarre, par Mr M. A. Pictet, - 137
Mémoire sur les Charagnes, par Mr. Vaucher, - 168
Mémoire sur les animalcules spermatiques de divers animaux, par MM. J. Prevost et H. Dumas, - 180
Mémoire sur les affinites naturelles de la famille des Nymphéacées, par M. Decandolle, - - - 208
De l'influence des fruits verts, sur l'air avant leur maturité, par M. de Saussure, - - - - 245

In a subsequent Number, we hope to be able to give an account of some of the most popular of the preceding memoirs.

53. *Translation of Legendre's Elements of Geometry.*—A translation of this classical and popular work on Geometry, which has gone through so many editions in France, is now in the press, and will be published in a few weeks. The work is edited by Dr Brewster, and under the sanction of M. Le Chevalier Legendre, who has communicated several important additions to the editor. As all the diagrams are engraven on wood, so as to accompany the propositions, this edition will possess a very great superiority over the original work, where they are given in copperplates at the end of the book.

54. *Mathematical Essays by the late William Spence, Esq.*—On the appearance of the first of these Essays on the Theory of the various Orders of Logarithmic Transcendents in 1809, its merits were universally acknowledged to entitle the author to hold a high rank among mathematicians. It is to be regretted that a premature death carried him off at a time when he was making such a rapid progress in the cultivation of the sciences. Of these tracts, the first only was intended by the author to meet the public eye in its present shape, though a few copies of another of them, denominated Outlines of a Theory of Algebraical Equations, had been printed and distributed among the author's friends. The others were selected from Mr Spence's papers by the editor, J. F. W. Herschel, Esq., who was pre-eminently qualified to judge of the respective merits of the several pieces. These papers clearly prove, that Mr Spence possessed both genius and industry in a high degree, and a force of intellect of a very superior kind. Notwithstanding the disadvantage attending some of the essays, in common with other posthumous works, we warmly recommend them to the attention of the British student, as models of analytical papers of uncommon excellence. They will inspire him with a taste for pure analysis, which, when properly directed, in the present state of the sciences, is the most certain method of arriving at distinction.—W. G.

55. *Memoirs of the Wernerian Natural History Society.*—The first half of the fourth volume of the Memoirs of the Wernerian Society, has just made its appearance, but not in time to allow of our giving any account of the individual papers. The following are the titles of the different communications. 1. On the Crystallisation of Copper-pyrites, by William Haidinger of Freyberg, member of the Wernerian Society. 2. Notice of attempts to reach the Sea by Mackenzie's River, since the Expedition of Sir Alexander Mackenzie. 3. Geological Notice and Miscellaneous Remarks, relative to the District between the Jumna and the Nerbuddah, by Dr Adam of Calcutta, member of the Wernerian Society. 4. Notice regarding the Fossil Elephant of Scotland, by Robert Bald, Esq. member of the Wernerian Society. 5. Description of Seven New Scottish Fungi, by R. K. Greville, Esq., member of the Wernerian

Society. 6. Meteorological Register kept at Clunie, in Perthshire, for twelve years, by the Reverend William Macritchie of Clunie. 7. Description of a new species of Grimmia found in Scotland, by R. K. Greville, Esq. 8. On the Geognosy of Germany, with observations on the Igneous origin of Trap, by Amié Boué, M. D., member of the Wernerian Society. 9. A new arrangement of the Genera of Mosses, with characters and observations on their distribution, history and structure, by R. K. Greville, Esq. and G. A. W. Arnott, Esq. 10. Short account of the Rocks in the neighbourhood of St John's, Newfoundland, by Mr John Baird. 11. Observations on the Snowy Owl (*Strix nyctea*), &c. by Lawrence Edmondston, Esq. of Shetland. 12. Meteorological and Hydrographical Notes, by Captain R. Wauchope, R. N. 13. Account of the small district of Primitive Rocks, near Stromness, in the Orkney Islands, by Mr George Anderson of Inverness. 14. Account of a new species of Larus, shot in Zetland, by Lawrence Edmondston, Esq. 15. Notice relative to two varieties of *Nuphar lutea*, found in a lake in Aberdeenshire, by Mr W. Macgillivray. 16. Geognostical Sketch of part of the Great Glen of Scotland, by Mr George Anderson of Inverness. 17. Observations on the Immer Goose of Zetland, by Lawrence Edmondston, Esq. 18. A Description of two new Plants, of the order *Algæ*, found in Scotland, by R. K. Greville, Esq. 19. Observations on the natural history and habits of the Mole, by Dr James Grierson, member of the Wernerian Society, and Minister of Cockpen. 20. Account of the Island of Foula, by Captain Vetch of the Royal Engineers, member of the Wernerian Society, &c.

ART. XXXIV.—*List of Patents granted in Scotland, from 5th February to 13th April* 1822.

7. TO JOHN RICHARD BARRY of the Minories, in the city of London, gentleman:—For an "invention of certain improvements on, and additions to wheeled-carriages." Sealed at Edinburgh the 1st March 1822.

8. To JOSEPH GRANT of Gutter Lane, Cheapside, London, Crape manufacturer:—For an "invention of a new manufacture of crape." Sealed at Edinburgh the 1st March 1822.

9. To **John Frederick Smith** of Dunston Hall, parish of Chesterfield, county of Derby, Esq. :—For " certain improvements in dressing of piece-goods made from silk or worsted, or of both these materials." Sealed at Edinburgh the 1st March 1822.

10. To **William Church** of Threadneedle Street, city of London, Gentleman :—For " invention of an improved apparatus for printing." Sealed at Edinburgh the 1st March 1821.

11. To **David Gordon** of the city and county of Edinburgh, Esq.—For " certain improvements on, and additions to steam-packets and other vessels, part of which improvements are applicable to other naval and marine purposes." Sealed at Edinburgh the 1st March 1822.

12. To **John Ruthven** of the city of Edinburgh, printer: —For an " invention of a new method of procuring a mechanical power, which will be of great public utility." Sealed at Edinburgh the 6th March 1822.

13. To **William Erskine Cochran** of Somerset Street, Portman Square, county of Middlesex, Esq.—For " certain improvements in the construction of lamps, whereby they are rendered capable of burning concrete oils, animal fat, and other similar inflammable substances." Sealed at Edinburgh the 13th April 1822.

14. To **John Pool** of Sheffield, county of York, victualler: —For " certain improvements in plating iron or steel with brass or copper, alloyed with other metal or metals, both plain and ornamental, for the purpose of rolling and working into plates, sheets, or bars, and such goods or wares to which the same may be found applicable." Sealed at Edinburgh the 13th April 1822.

15. To **John Slater** of Birmingham, county of Warwick, manufacturer :—For " invention of a kitchen-range and apparatus for cooking, and other purposes." Sealed at Edinburgh the 13th April 1822

16. To **George Stratton** of Hamstead Road, county of Middlesex, Enginee[illegible]or an " improved process of consuming smoke. Seale[illegible] the 13th April 1822.

P. Neill, Printer.

THE

EDINBURGH

PHILOSOPHICAL JOURNAL.

ART. I.—*On Black Lead from Cast-Iron.* In a Letter to Dr BREWSTER from J. MACCULLOCH, M. D. and F. R. S., &c. Communicated by the Author.

DEAR SIR,

IN compliance with your request, I send you the following statement respecting the Black Lead which is obtained from cast-iron, various detached notices of which has already appeared in your Journal.

It is more than twenty years since I was in the frequent habit of examining the metal of the iron guns delivered by the contractors to the Ordnance, by solution, according to Bergman's suggestion on this subject. No useful results, as far as our particular objects were concerned, were, however, obtained in this way; as it was found that the quantity of plumbago in the iron bore no relation to the strength or goodness of the metal, which, I need not say, is metal of the second fusion, or from the reverberatory. The results were, however, sometimes curious, from the very variable quantity of this substance contained in different specimens of iron.

It has been imagined that the worst, or weakest metal, was that which contained most plumbago; but the trials were far from confirming this opinion. On one occasion, in particular, a gun had been condemned for some fault in the bore, (a screwhole, I believe,) and it was found to be so exceedingly tough, that none of our men were able to break off a trunnion, as is the usual practice in these cases, when three or four blows of the sledge were commonly found sufficient for this purpose. I was particularly induced, therefore, to examine this specimen, and was surprised to find that it not only contained a most unusual proportion of plumbago, but that this was in what I may call a disengaged state, for want of a better term. The plumbago was not only visible on breaking the metal, giving it the appearance of having been rubbed with powdered black lead, but the iron was capable of leaving its trace on paper. I have neither before nor since ever met with another example of this kind. The remaining guns of this lot, which must have been made of the same metal, went on service, and some of them are probably still existing. I have no doubt that they were the best that we received during the war; and it would have been very desirable to have discovered by what means this very uncommon specimen of gun-metal had been procured, as its toughness is a matter of such importance. It was from Mr Walker's foundery.

I do not pretend to account for this singular state of the iron; as, although the presence of plumbago is sufficient to affect the colour of the metal, it is never, bating this instance, distinctly visible, as far as my experience goes. Yet, in gun-metal, it is easy to conjecture its presence and proportion, by the colour of the fracture; which is darkest or most grey, where it is most abundant. The trials which I have made, also go to prove that the greyest metal is the toughest; although I know that many iron-founders consider that gun-metal may be too grey, and act on this principle in the assortment of the pigs for the reverberatory.

I ought to add now, that, on dissolving gun-metal in an acid, the black lead was always obtained in the state of powder, and that it is in very small proportion when compared to that obtained from pig-iron. I am sorry that I cannot now state these proportions, as the records of my experiments are out of reach at present.

It is perhaps un [illegible] that the inferior proportion of plumbago in gun [illegible] m its approximation nearer to the malleable state than pig-metal, or from its greater purity. The best malleable iron contains none, and a great part of the process of refining iron consists in the combustion of this substance; whence in some degree the loss of weight.

During these trials I was led to repeat the same experiments on the pig-metal used for shells, which is of various qualities, and is generally distinguished into three kinds, white, grey, and black. All these afforded very large proportions of black lead after solution, but still various in different specimens. The black metal seemed to afford the greatest quantity, as might have been expected from the colour; and yet that is no criterion, as the most brilliant silvery metal also afforded it in abundance, where, if colour had been a necessary indication of its presence, it would not have existed. I think it will appear that the plumbago is present in two different states in iron. It will hereafter be shewn, that it is sometimes in the state of a metal, and this I conceive to be its form in the white pig. At the other extreme it is found in the state of black lead, as in the gun-metal just mentioned. But the experiments hereafter to be described will shew, that there is some intermediate state (or states) between the two; and this appears to be its condition in the grey and black pig.

I must now observe, that, in these experiments on pig-iron, the black lead was always found in the state of powder, or at least in such a state as to fall into powder easily. This must have arisen from using sulphuric acid, as the cheapest, and from using it too strong. Thus the matter rested. In 1807 or 1808, I was requested to go to a London porter brewery, to see an appearance which had very much surprised the people employed in it, and which had certainly a very marvellous sound in the narration. The workmen had taken some iron out of their porter backs in making some repairs, and had found it red hot, as they said, to their infinite astonishment. On closer examination, the red heat was found to be an exaggeration. But, on removing the iron articles in question from the porter, they became so hot on scraping off the surface that it was disagreeable to handle them; while they smoked, from the evaporation of the moisture.

These pieces of iron were cast cones, perforated with holes, and about an inch thick, being used as strainers, to prevent foreign substances from getting into the pipes. They had been immersed in the porter for many years,—no one knew how many. On examining them, they appeared entire and unchanged; but some of them, instead of being iron, were entirely of black lead; and, in others, there was a thick coat of that substance on each side, a little iron only remaining in the middle.

As this was then a new phenomenon in chemistry, I was induced to repeat the experiment in the laboratory, and it was attended with the same results, after some failures. An accident, however, occurring about this time, shewed me that the fact was not so new as I had imagined, and I therefore took no notice of it in public. Since that it has been observed by many persons, who all probably imagined, with as good reason, that they had made the same discovery. As many of these accounts have appeared in your own Journal, I need not refer to them more particularly.

The circumstance to which I allude was the following, which I happened to meet with in one of my journeys in the Western Islands.

After Captain Roe had invented the diving-bell, he joined Sir Archibald Grant, a great speculator of that day in coal-mines and other matters, in an attempt to weigh the Florida, one of the Spanish Armada, which had foundered off the coast of Mull, near the entrance of Tobermory harbour; she having been taken by the natives, assisted by some treachery on the part of the crew, which is said to have hung up the Captain at the yard-arm. There her timbers are still lying.

This attempt, which took place in 1740, was unsuccessful, as far as related to the ship; but some guns both of brass and iron were brought up. The former, whether they belonged to the Spanish vessel or not, had the mark of an English founder, R. and J. Phillips, 1584, with a crown and E. R. on them. The iron guns were deeply corroded, and on scraping them, it was said that they were found so hot that they could not be touched, and that they did not become cool till they had been two or three hours exposed to the air.

The astonishment of the Highlanders on finding guns still hot, after having been more than a century under water, may easily be imagined; and it is not surprising that the story was not believed, and that not being believed, it was forgotten. This may afford us an useful hint on the subject of physical incredulity: since, assuredly a fact thus nakedly stated, without a knowledge of the explanation here given, must have been pronounced impossible by every one, chemist or not. I escaped this, however, as the circumstances above mentioned had put me in possession of the solution before the tale, which proved a very agreeable and unexpected confirmation of my own experiments.

I may now state the general result of the experiments. The blackest pig-metal appears to yield the greatest quantity of black lead, and in the most solid state. When the experiment is complete, the produce equals the iron in bulk, and is a solid mass, capable of being cut by a knife, even into pencils; but, as far as I have ever observed, it is of a much more coarse grain, or scaly granular texture, than any natural black lead that has occurred to me.

To procure it in perfection, the acid should be very weak, and the operation is then necessarily very tedious. Acetous acid appears to be the best, and it is by this that it is produced in porter-backs, in the waste-pipes of breweries, and in calico-printing-houses, where sour paste is employed. The process by water is insufferably tedious. Very dilute mineral acids also succeed; but, with these, one of the results is sometimes not obtained.

If the experiment is perfect, the black lead becomes hot on exposure to air, smoking while there is any moisture to be evaporated, particularly when the surfaces are scraped off in succession, so as to give access to the air. Oxygen is absorbed in this case; yet, as far as I have perceived, the eye cannot detect any difference in the appearance of the black lead before and after this operation. In those instances where the substance does not heat, on being taken out of the fluid, it appears to arise from the whole process of oxygenation having been performed in the solution, and probably from an excess of strength in the acid.

The theory of this experiment appears very plain, and it proves, with tolerable certainty, what has been supposed, but what has not yet been proved in any other way, namely, that plumbago is a metal, and black lead its oxide, if I may be allowed to use that term for the present, instead of carbon.

In white-pig, as already suggested, it is probable that the combination is pure plumbago and iron. In the black, the colour would lead us to imagine, that there was already an approximation to black lead. The operation of the acid, in either case, is to dissolve the iron, and to oxygenate the plumbago, so as to convert it into black lead. Thus, when in small quantity, it is obtained in the form of powder, when very abundant, in a solid state. If the acid is strong, the whole operation is completed in the solvent; otherwise some additional oxygen is required to produce in it a state of rest or permanence in the air; and this takes place by a species of combustion, generating the heat in the experiment, analogous to that which occurs with the alkaline bases.

Thus, black lead is an oxide of plumbago, or of carbon, if we choose to use this term for the presumed element. It is scarcely necessary to say that the metallic nature of the base of charcoal is proved by the same experiment. Nor need I say that iron is not a necessary ingredient in black lead. The best kinds, indeed, are those which contain least.

This experiment, and these conclusions, would be much more satisfactory, if we could produce the metal of black lead in its separate state. No method of doing that has yet occurred to any one; and it will probably be found a very difficult problem, as this is evidently a highly combustible substance. But chemistry does so much every day that once appeared hopeless, that we have no reason to despair.

If the foregoing reasoning respecting the metallic nature of this substance, should be deemed unsatisfactory, the following argument may be added.

The specific gravity of pig-iron is about 7.6, and that of black-lead is 2, or less. Now, the bulk of black-lead procured in this experiment, is equal to that of the original iron exposed to solution. Two such bodies could not co-exist in the same space; or, if that could be imagined, the specific gravity of such iron must be far more different from that of pure iron than it is. If pure iron, indeed, is freed from the effects of condensation by heating, it scarcely differs in specific gravity from pig-metal. Thus, while we conclude that the plumbago combined with the iron is a metal, we may also infer, that the specific gravity of that metal is not very different from that of iron.

A problem has long been held out, namely, that of making black-lead by an artificial process. This may be considered as now solved in a certain way. I attempted to improve its quality by heating, in the usual manner, but with indifferent success. It may be procured, however, so compact, as to cut into pencils, as formerly observed; but I never could free it from the brown colour which it gives to paper, arising from some rust which it retains. Whether this could be effected or not, seems a matter of indifference, as far as economical purposes are concerned; since it could not be produced so cheap in this manner, as to compete in the market with mineral black-lead.

I may conclude this communication with remarking, that the effects of heat on black-lead, as well as on charcoal, are, in some measure, explained by the views here held out. If soft black-lead, which yields a black as well as a soft streak, be heated without burning, it diminishes in bulk and increases in specific gravity, while it becomes hard and gives a pale streak. This is the process used in making hard and pale pencils; and it appears to consist in the loss of a portion of oxygen, or in the reduction of the black-lead, to a state somewhat nearer to that of metallic plumbago. Thus, in drawing, artists harden the points of their soft pencils in the flame of a candle.

Something very similar to this happens in charcoal. After giving out hydrogen it becomes harder and more brilliant; so as, from only scratching the softer metals, to become capable of corroding glass. In this state it loses one at least of its valuable qualities, so as to be no longer fit for making gunpowder. Naturally, some woods produce hard charcoal, and others a soft kind; and the latter alone are fit for this purpose. But the softest may be injured by over heating, or they can be reduced to this hard state; so that the wood of the willow or alder may become as bad as that of the oak. This is an accident far more likely to happen in the method of charring in retorts, than in the common way of making charcoal in pits; although that process is always used in the gunpowder manufactories. It is for the same reason that animal charcoal, or that of coal, will not make gunpowder. Both of these are hard and brilliant; and the former, in particular, has sometimes a pseudo-metallic lustre, equal to that of black-lead. The chain of analogy is thus extended among all these substances:—but it is time to terminate this communication. I am, yours, &c. &c.

EDINBURGH, *July* 1822. J. MACCULLOCH.

ART. II.—*Account of a Man who lived Ten Years after having Swallowed a number of Clasp-Knives.* By ALEXANDER MARCET, M. D. F. R. S. late Physician to Guy's Hospital *.

IN the month of June 1799, John Cummings, an American sailor, about twenty-three years of age, being with his ship on the coast of France, and having gone on shore with some of his shipmates, about two miles from the town of Havre de Grace, he and his party directed their course towards a tent which they saw in a field, with a crowd of people round it. Being told that a play was acting there, they entered, and found in the tent a mountebank, who was entertaining the audience by *pretending* to swallow clasp-knives. Having returned on board, and one of the party having related to the ship's company the story of the knives, Cummings, after drinking freely, boasted that he could swallow knives as well as the Frenchman. He was taken at his word, and challenged to do it. Thus pressed, and though (as he candidly acknowledged in his narrative) "not particularly anxious to take the job in hand, he did not like to go against his word, and having a good supply of grog inwardly," he took his own pocket-knife, and on trying to swallow it, "it slipped down his throat with great ease, and by the assistance of some drink, and the weight of the knife," it was conveyed into his stomach. The spectators, however, were not satisfied with one experiment, and asked the operator "whether he could swallow more?" his answer was, "all the knives on board the ship;"

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* Abridged, with the Author's permission, from the Twelfth Volume of the *London Medico-Chirurgical Transactions*.

upon which, three knives were immediately produced, which were swallowed in the same way as the former; and "by this bold attempt of a drunken man," (to use his own expressions), "the company was well entertained for that night." The next morning he had a motion, which presented nothing extraordinary; and in the afternoon he had another, with which he passed one knife, which however was not the one that he had swallowed first. The next day he passed two knives at once, one of which was the first, which he had missed the day before. The fourth never came away, to his knowledge, and he never felt any inconvenience from it. After this great performance, he thought no more of swallowing knives for the space of six years.

In the month of March 1805, being then at Boston in America, he was one day tempted, while drinking with a party of sailors, to boast of his former exploits, adding, that he was the same man still, and ready to repeat his performance; upon which a small knife was produced, which he instantly swallowed. In the course of that evening he swallowed five more. The next morning crowds of visitors came to see him; and in the course of that day he was induced to swallow eight knives more, making in all fourteen.

This time, however, he paid dearly for his frolic; for he was seized the next morning with constant vomiting, and pain at his stomach, which made it necessary to carry him to Charleston hospital, where, as he expresses it, "betwixt that period and the 28th of the following month, he was safely delivered of his cargo."

The next day he sailed for France, on board a brig, with which he parted there, and embarked on board another vessel * to return to America. But during her passage, the vessel, which was probably carrying on some illicit traffic, was taken by His Majesty's ship the Isis, of 50 guns, and sent to St John's, Newfoundland, where she was condemned, while he himself was pressed and sent to England on board the Isis. One day, while at Spithead, where the ship lay some time, having got drunk, and, as usual renewed the topic of his former follies, he was

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* The "Betty" of Philadelphia.

once more challenged to repeat the experiment, and again complied, "disdaining," as he says, "to be worse than his word." This took place on the 4th December 1805, and in the course of that night he swallowed five knives. On the next morning the ship's company having expressed a great desire to see him repeat the performance, he complied with his usual readiness, and, "by the encouragement of the people, and the assistance of good grog," he swallowed that day, as he distinctly recollects, nine clasp-knives, some of which were very large; and he was afterwards assured, by the spectators, that he had swallowed four more, which, however, he declares he knew nothing about, being, no doubt, at this period of the business, too much intoxicated to have any recollection of what was passing. This, however, is the last performance we have to record; it made a total of at least thirty-five knives, swallowed at different times, and we shall see that it was this last attempt which ultimately put an end to his existence.

On the following day, 6th of December, feeling much indisposed, he applied to the surgeon of the ship, Dr Lara, who, by a strict inquiry, satisfied himself of the truth of the above statement; and, as the patient himself thankfully observes, administered some medicines, and paid great attention to his case, but no relief was obtained. At last, about three months afterwards, having taken a quantity of oil, he felt the knives (as he expressed it) "dropping down his bowels;" after which, though he does not mention their being actually discharged, he became easier, and continued so till the 4th of June following (1806), when he vomited one side of the handle of a knife, which was recognized by one of the crew to whom it had belonged. In the month of November of the same year, he passed several fragments of knives, and some more in February 1807. In June of the same year, he was discharged from his ship as incurable; immediately after which he came to London, where he became a patient of Dr Babington, in Guy's Hospital. He was discharged after a few days, his story appearing altogether incredible, but was re-admitted by the same physician, in the month of August, his health during this [illegible] having evidently become much worse. It was probably at t[illegible] he unfortunate sufferer wrote his narrative, which [illegible] second admission into the hospital. I find, however, by the hospital records, that, on the 28th of October, he was discharged in an improved state; and he did not appear again at the hospital till September 1808, that is, after an interval of nearly a year since his former application. He now became a patient of Dr Curry, under whose care he remained, gradually and miserably sinking under his suffering, till March 1809, when he died, in a state of extreme emaciation.

The following interesting letter from Dr Lara, (probably to Dr Curry, as it was found among his papers), supplies some of the particulars respecting the patient's illness while on board the Isis.

" Sir,

" William Cummings, aged twenty-nine years, a seaman belonging to H. M. S. Isis, was reported " sick" to me, as surgeon to that ship, on the 6th of December 1805. He complained of excessive pain in the stomach and bowels, incapacity of retaining any thing on the stomach, and severe pain in walking or standing erect. These symptoms he attributed to having swallowed, during the three preceding days, nineteen or twenty pocket-knives, and one paper knife-case; the latter he stated to have been presently returned, but all the former retained!

" Incredulous of this statement, I made every possible inquiry; and, on the evidence of those who solemnly declared they witnessed the fact, the number of knives actually taken into the stomach appeared to be fourteen! The greater part of these knives were nearly four inches long, and full one inch in their extreme breadth. It seems, he sought no remuneration for this extraordinary exploit, but a plentiful supply of what is termed " grog" at sea, *i. e.* spirits and water; nor did he apprehend any attendant danger, as he had (he said), a few years previously, swallowed eighteen knives at Boston in America, of which he got rid in four days, without the least inconvenience.

" I must here observe, that an inhabitant of Boston, whom I met at St John's, Newfoundland, in the summer of 1806, assured me, that the fact of a man named William Cummings having swallowed several knives was well remembered by many inhabitants of Boston, and that a number of knives said to have been expelled by this man were still preserved in the Infirmary of that city.

How to act in this unparalleled case, I confess I knew not. He expressed himself certain, (having frequently, he averred, swallowed one or two knives in a day, and as often discharged them, without inconvenience) that every knife had passed the stomach; and though it might not have been so (as is since proved), any attempt to force them up appeared to me to be attended with much danger; nor did I suppose it more expedient or less hazardous to attempt their expulsion downward. I trusted, therefore, to the efforts of nature, merely directing some castor-oil to be given at intervals, with or without opium, according to the degree of pain, and urgency of the vomiting, which immediately occurred on his sitting up, or swallowing any thing solid. Glysters of thick water-gruel were also frequently injected.

"In about a week from his first application, the vomiting was less frequent, and the matter ejected of a lateritious colour; the stools were black and thin. Some medical friends now suggested a trial of the sulphuric acid, in which I acquiesced. For a fortnight he took thirty or forty drops of the diluted acid four or five times a day, with tincture of opium at intervals, and a gentle laxative when costive. His diet during this time was sage, rice, tea, bread, cheese, and beef-soup, most of which were retained. The matter ejected from the stomach had gradually acquired a darker colour, as if impregnated with iron, or mixed with ink. The stools were as before described. The pulse continued unaffected; but he was evidently emaciated.

"Notwithstanding these appearances, a general disbelief prevailed as to the cause to which this poor man attributed his complaints, and which disbelief was strengthened by several, who had before declared they had seen him swallow the knives, prevaricating in their evidence on being repeatedly examined. I was also blamed for keeping the man from his duty, and charged with favouring an impostor. I would not, however, discharge him. At this time my senior assistant, Mr Thomas Watts, requested permission to try the effects of the muriated tincture of iron, conceiving, that if the system was brought under its influence, the expulsion of the knives might be proved.

I consented to the trial, and Cummings took ten drops every four hours, daily increasing each dose by a drop, for a fortnight. No effect was however produced, except tormina, which symptom opium did not relieve so readily as heretofore; his appetite continued good, except for animal food. Under these circumstances, he was directed (having hitherto kept in bed) to sit up the whole of the day. For a few days he always vomited on rising; but within a week he lost that symptom, and the vomiting seldom occurred oftener than three times in the twenty-four hours, and then mostly after taking tea or other liquid. Though these fluids were returned a very few minutes after they were swallowed, the matter vomited had always the appearance of ink and water. The exhibition of any medicine was now suspended, except for the obviating of costiveness.

" Towards the close of January 1806, he was evidently more emaciated; yet he moved about, and at intervals performed the duty of a sweeper. Until the 26th of the following April, there was but little alteration: he then complained of much pain, and incessant vomiting. Opium, the warm bath, and laxatives were employed, but without any apparent advantage; yet, at the end of May, the vomiting was much less in quantity and in frequency; the pain was also relieved, being felt only on his attempting to stand erect; his appearance was also improved, and he was discharged to the performance of such light duties as he could execute.

" On the 6th of June, he brought to me the half of a horn handle of a knife, which he declared he had thrown off his stomach that morning, without any particular effort or attending pain.

" Nearly five months now elapsed without any particular occurrence. He gathered strength and flesh, ate voraciously, drank proportionably, and performed various easy duties in the ship; though, when questioned, he complained of pain when standing erect, and of vomiting at intervals.

" On the 8th of November, he passed, in two alvine evacuations, the blade, and half the horn handle of a knife; he then complained of much pain in the abdomen, and was relieved by a dose of castor-oil combined with tincture of opium. The 12th of November he passed another portion of iron, with much pain, but which subsided on the expulsion being effected. The 30th of November I was removed from the Isis; at which time Cummings expressed himself to be easier than he had long been, though suffering from pain, and vomiting at intervals.

"The surgeon who succeeded me (Mr Peter Kelly) wrote to me, on the 23d of January 1807, to say that Cummings was much the same as when I left him, and had passed, in the early part of the month, the iron portion of the handle of a knife. In May 1807, I received a farther communication from Mr Kelly. He stated, that Cummings continued much as usual; that he had passed several pieces of iron, one of which had excited extreme pain, from its having lain transversely on the rectum; and that one piece, with a hook-like end, had been ejected from the stomach, with excruciating pain, and hæmorrhage to the amount of two pounds of blood. Mr Kelly further states that the expulsion of the different pieces of iron was gerally preceded by severe pain in the abdomen.

"After the last communication from Mr Kelly, I never heard any thing further respecting Cummings, until I received your favour of the 22d instant. If the incomplete account I have here given of this extraordinary case shall prove at all satisfactory, I shall be satisfied. I have only now to add, that of the portions of knives expelled, I have six pieces of iron, and two of horn. I trust you will have the goodness to favour me with an account of the examination of the stomach; in the expectation of which, I remain, Sir,

Your very humble servant,

B. LARA."

The following very curious narrative was drawn up by Cummings himself, and entitled, "A miraculous Recovery of a Seaman, who swallowed a number of Knives at three different times, as you see in this little book;" and though it contains most of the particulars already stated, we shall follow Dr Marcet in giving it in Cummings' own words.

"John Cummings, thirty-two years of age, was, in the month of June 1799, in France. Upon a Sunday afternoon, he and a party of his shipmates went out to the country, two miles from the town of Havre de Grace, where, upon their approaching a large field, observed a great number of men and women standing, and a large tent that was placed upon the ground. Being anxious to know what they had been about, steered their course towards the place where the multitude stood; by inquiring of the first they met with, was informed it was a play. Directly they collected a livre each, and entered inside the scenes. They had full view of the performance; but by observing the play-actors swallowing knives, induced him to try the experiment. That night they were in time enough on board, where they began to enjoy the former part of the night as follows:—After drinking very hearty, one of the company opened the story concerning the above play-actors, which he repeated that it was an extraordinary affair to swallow knives. The author made answer directly, and told him that he could swallow knives as well as they could. The company present took notice of the above answer being made so quick, and for the curiosity of the circumstance, made a serious enquiry if he was man enough to perform what he had already stated. He did not like to go against his word, neither was he anxious to take the job in hand; but, by having a good supply of grog inwardly, he took his own pocket-knife, and tried it first, which slipped down his throat with great ease; and, by the assistance of some drink and the weight of the knife, conveyed it into his stomach. But still the spectators seemed not satisfied with one, but made further enquiry, if he could swallow any more. He replied in a word, "All the knives on board the ship." By this answer, there was three more knives presented upon the table, which he swallowed in few minutes, the same way as the former. And by this bold attempt of a drunken man, the company was well entertained for that night. Next morning nature worked him to a stool, but passed nothing extraordinary; at four o'clock the same afternoon had another, when he passed one, and, what was more surprising to him, that was not the first knife he swallowed; the next day he passed two at once, and one of them being the first knife he swallowed; the fourth, never knew any thing about it, nor ever knew whether it came away or remained in his bowels. But yet he never suffered pain by them that time, and was safely delivered without any assistance from a surgeon; and shortly afterwards took his departure from France, and never thought on swallowing any more knives for the space of six years; after which, you shall see as follows:

"Boston, March 13. 1805, was in company, where he gave his report of his success in swallowing knives in France, in June 1799. Two or three of the company told him plain to his face, that it was impossible for any man to do such a thing, and that it was nothing but false report, which he took it very highly affronted; but, after considering a short time, told the company he was the same man still, and, if it was agreeable, that he would satisfy their curiosity. One small knife was presented to him, which he swallowed instantly; in the course of that night he swallowed five more, which made six in all. This uncommon report soon came to a head in the neighbourhood, as he had next morning a thousand visitors; but he gave very few of them admittance. Happened in the course of that day that he swallowed eight more, and six the night before, which made fourteen; and that was the 14th of the month; so he had swallowed a knife for every day the month was old. Next morning was the 15th; he was taken very ill, with constant vomiting and pain in his stomach. Directly he was brought to Charleston Hospital, and betwixt that period of time and the 28th following, was safely delivered of his cargo, and the whole of them are preserved in the Infirmary of that city. Upon the 29th he shipped himself on board of a brig bound to France, where he left that brig, and shipped on board of the Betty of Philadelphia; in their passage to America, was taken by his Majesty's ship Isis, fifty guns, and sent into St John's in Newfoundland, where she was condemned, and some of the hands pressed. Being a few months laying there, got orders to sail for England. After twenty-five days agreeable passage, made the land, and came-to at Spithead; being there for some days, and got on board fresh provisions of all kinds, and amongst that plenty of spirituous liquors, after drinking very hearty, he told of his success in swallowing knives in France and in America. None of his shipmates would give credit to his history, which some body present was inhuman enough to offer him a knife. Upon disdaining to be worse than his word, he proceeded immediately to perform his part of the business; that night being the 4th of December 1805; and in the course of that night he swallowed five knives. Next morning, being the 5th day of the month, the ship's company was anxious to see the performance renewed the second time: by the encouragement of the people, and the assistance of good grog, (and his lot was ordained to be miserable hereafter in consequence of the same), he swallowed nine that day to his own knowledge; and the spectators informed him afterwards that he swallowed four more, that he knows nothing about: they were all clasp-knives, and some of them very large. Upon the 6th of December he was under the necessity of applying to a doctor, who was surgeon of the ship. The doctor, finding he was in a bad situation, made a strict enquiry of the principal men that were eye-witness to the transaction, which the captain and the rest of the officers found to be a true story. The surgeon, indeed, never neglected to pay the greatest attention, and prescribed what medicines he thought proper towards his relief; but all to no effect. At the expiration of three months, by taking a quantity of oil, he felt them dropping down to his bowels. In a few days after he was able to walk any part of the ship, and in that continuance till the 4th of June following, when he vomited one side of the handle of a knife, marked Cunningham, the same man that it belonged to, in the same ship; and, by asking him if he knew any thing about such a knife, he directly confessed that it was part of the knife Cummings swallowed of his. The surgeon keeps the said piece in his possession. Four months passed without any thing extraordinary having happened. On the 4th of November he passed another piece, the same as the former, with the lining of a knife along with it; two more he passed during that month. In February following vomited another lining of a knife; in the course of that month passed four more pieces, and since nothing extraordinary came away.

"June 12. 1807, he was discharged the ship, in consequence of his complaint, and likewise being found, at the survey, unserviceable; after which, he was admitted into Guy's Hospital, under the care of Dr Babington. Great many never believed such a circumstance. After five weeks being in the hospital, was presented out, and was in lodgings for the space of five weeks; but, finding himself getting worse, was obliged to make the second application, and was re-admitted under his physician again."

Art. III.—*On the Use of the Thermometer in Distillation, as an Alkohometer* *. By M. F. Groening.

Mr F. Groening in Copenhagen, in the course of using an apparatus for distilling, invented by himself, made the following interesting and useful discovery of the use of the thermometer in distillation, as an alkohometer.

At first he used the thermometer merely for comparing the temperature of the interior of the rectifier, with that of the water placed about it.

After many experiments made with this view, he observed, without variation, that the thermometer always rose to a certain point, for example 65° Reaumur, before the first drop of the distilled liquor appeared; and, likewise, that it remained at that point till about half the fluid in the retort was evaporated, but then, by degrees, at first slowly, afterwards more rapidly, rose to 8° R.

This discovery led him to the conclusion, that the phenomenon could only depend on the different temperatures of the spirituous and watery vapours. The former, which comes over first, has, as is well known, a lower temperature than the latter, which follows after the spirit has been extracted, and thus the thermometer is affected by each in succession.

To reap more benefit from this discovery, at each change of the thermometer, he tried the distilled liquor with the alkohometer, by which means he obtained the following very interesting result: that as long as the thermometer remained at a certain point, the liquor which came over was of an uniform strength, but when the thermometer rose, the liquor grew weaker and weaker, till at last mere water came over, namely, when the instrument had attained the height of 80° Reamur.

The following table, constructed from many corresponding results, will serve to convey a clearer idea of what has been said.

In a vessel of the capacity of 30 pots (rather more than a quart English to each pot), were placed 25 pots of brandy, of into an adjoining apartment, for the person who attends the work does not require the thermometer to direct him.

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* We have been indebted for this account of M. Groening's Alkohometer to W. C. Trevelyan, Esq. who translated it from the original Danish.—Ed.

With a larger apparatus of 200 pots, the thermometer differed much from the preceding scale, which probably was caused by its not being perfectly free from air; this, however, does not refute the accuracy of the above-mentioned results. A person may, at the first trial with each apparatus, immediately form a scale for himself.

Although this discovery has only been tried with Mr Groening's apparatus, it is nevertheless his opinion, that a proper application of the thermometer to an apparatus, of whatsoever construction, will give the same results.

ART. IV.—*Additional Observations on the Chinese Lusus Naturæ.* By JOHN LIVINGSTONE, Esq. Surgeon to the British Factory, China. Communicated by the Author *.

ON the 19th and 20th of September 1821, I had full opportunities of examining A-ke (now known by the name of A-gan), who appears to be a stout active young man, now seventeen years of age, and notwithstanding the incumbrance of his parasitic brother, he is able to do the work of a husbandman; but while he is at work, he is obliged to suspend his brother. The natural attachment of the parasite is from his sternum, in a line with his fourth rib; it continues downwards to within about an inch of his umbilicus. The upper part consists of the four lower cervical vertebræ, of which the spinous processes of the upper and two lower can be distinctly felt. The integuments extend quite smoothly from the chest of A-gan to the neck of his brother, and the subcutaneous bloodvessels appear of the natural size. The breadth of the upper part of this attachment does not exceed two inches, and the connection of the parasite's cervical vertebræ, to the sternum of his brother, admits a little rotatory motion, from the point downwards, to within about an inch of A-gan's umbilicus. The attachment is formed by the integuments, which gradually become thinner, allowing a more extensive separation of the parasite from the principal, till the lower portion consists of the duplicature of the skin, only admitting a separation of four or five inches. From the top to the bottom this connection measures eight inches and a quarter.

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* See this *Journal*, vol. V. p. 132.

The humerus of the parasite measures twelve inches, and is equal in length to that of the principal; but the bones of the fore-arm, though perfect, are not quite half so long as those of A-gan. The wrist, though small, is perfect. The hands are diminutive also, and the thumbs want the metacarpal bone. The arms can be moved freely. The right articulation of the scapulæ is perfect, and the lower costa extends about an inch and a half. The acromion process of the left arm can be felt attached to the humerus, and, though obscurely, a small part of the humeral portion of both clavicles, and notwithstanding the scapulæ and claviculæ are nearly wanting, and that there is neither sternum nor ribs, the arms remain opposite to each other, and can be moved with great freedom.

Instead of a chest and abdomen, the upper are connected with the lower extremities, apparently by the common integuments, strengthened by two firm tendinous bands, which may be supposed to occupy the place of the recti muscles, arising on each side of the connecting substance, between the two bodies, and are inserted deeply within the capacious pelvis of the parasite. The back part of the spineless bag, which is in place of a trunk, is in its natural position scarcely two inches in length, but which may be easily stretched to double that distance. The *ossa innominata* form a semicircular, smooth, upper margin, somewhat of the form of a horse-shoe; and if the horse-hoof is supposed to be connected with this, a good idea may be formed of this part of the monster. It is very large and prominent, and would doubtless weigh more than the same part of A-gan. The pelvis, as well as the chest and abdomen, appeared flaccid, and no contents could be felt.

The acetabula of the *ossa innominata* are perfect. The thigh bones of the parasite are well formed, and of nearly the same length as his brother's, viz. twelve inches. The remaining bones of the lower extremities appear about half their natural size, only three toes appear on one foot, on the other they adhere; but the bones can be distinctly felt.

Utriusque fratris genitalia perfecta sunt, atque illa parasiti, nisi quod est phymosis naturalis glandem tegens, et scrotum ita contractum ut testes non manifesto patere sinat, penitus evolvi videantur. Singulis subinde tentigines adsunt, sed neutro illorum adhuc aliquid seminis emissum est.

Parasitus haudquaquam constanter urinam suam eodem quo A-gan tempore reddit. Nonnunquam humor aliquanto turbidus stillat, quantitas cujus usu liquidorum copiosiore augetur.

A weak pulsation could be felt on the neck, arm, wrists, &c. of the parasite, by comparing which with the pulse of A-gan, no doubt can be entertained but one heart serves both.

A-gan states, that when very young, an attempt was made to remove the parasite by ligature, and that it was continued for five years, when his life appearing in great danger, it was then abandoned. It seems probable, that this treatment of the parasite may at least have modified his present condition; besides the necessary support when at work, had kept the legs of the parasite so long, and so closely bound up on the thighs, that an adhesion has been formed from the ham to the middle of the leg and thigh respectively. No anchylosis seems to have taken place.

He says, that at an early period, no adhesion existed, and then the parasite could move both arms and legs. In a subsequent conversation, he said he doubted this; but most strenuously maintained, that he feels every hurt inflicted on the body of his brother, even the bite of a musqueto: he complained of the pain, which he said was inflicted by pressing on his empty chest, trunk or pelvis. When I pinched unobserved his hip, he appeared unconscious that I had done so.

My friend Dr Morrison procured for me ample opportunities of observing this interesting *Lusus Naturæ*, and kindly gave me his best assistance in separating the truth, such as it has been stated, from much fable, which doubtless had been contrived to amuse spectators. The result differs in some respects from my printed account,—the principal facts remain, but it was obviously my duty to st[illegible] discrepancies that have appeared.

John Livingstone.

ART. V.—*Some Account of those Manuscripts in Great Britain, which contain the Greek Text of the Mathematical Collections of Pappus.* By S. P. RIGAUD, Esq. M. A. F. R. S. Savilian Professor of Geometry, and Professor of Experimental Philosophy in the University of Oxford, &c. &c. Communicated by the Author. (Concluded from p. 64.)

THERE is a manuscript at Edinburgh, for a description of which I must be indebted to Dr Trail, who gives a full detail of all the particulars of it in his life of Dr Robert Simson. He says that it contains "five books of Pappus, viz. the 3d, 4th, 5th, 6th and 8th; but unfortunately the 7th, the most valuable portion of the work, is wanting."

"This MS. was purchased at Paris in 1748, by Dr James Moor, then Greek Professor at Glasgow, from Mr De Mairan, of the Academy of Sciences; and on the first page of it is written D'Ortons de Mairan, probably by his own hand. De Mairan had informed Dr Moor that it had belonged to Bullialdus; and on that account Dr Simson, who had it for some time in his possession, and took many notes from it, calls it Codex Bullialdi. Like the other MSS. of Pappus, it abounds with errors; but Dr Simson obtained from it several improved readings and corrections, which he has remarked in his copy of Commandine's Translation. Some of them are curious and important, and all of them merit the attention of any future editor of that work.

"Some peculiarities in this MSS. may be mentioned, for the satisfaction of those who may be inclined to consult it.

"At the end of the 3d book are five pages of MS., not in Commandine's edition, which contain some variations of the solution of the Deliacal problem, in addition to those contained in the beginning of that book *.

"* It may be observed, that the Savil MS. No. 3., has a similar addition at the end of the 3d book." This same additional passage is found also in No. 9. Kollarius likewise describes it in his account of the two Vienna Manuscripts of Pappus: and, consequently, it must be in No. 2440. of the French King's Library, from which one of them is copied. It may not be wholly irrelevant to point out here a singular oversight of Kollarius, in his description of LXVII, the latter part of

" At the end of the 4th book, is a very imperfect sketch of a proposition not in Commandine.

" In this MS. are many corrections on the margin, and some even in the text, by a later hand; and among them are a great number of the emendations proposed by Commandine, of the very same errors in the MS. used by him, from which a connection between these two MSS. may be inferred. Many of these emendations are servilely copied, retaining even the mistakes into which Commandine, in a few of them, had fallen.

" This MS., though elegantly written, has been copied by a person totally ignorant of the subject, of which the number of gross errors in its first state is a sufficient proof. It may be remarked, also, that at the beginning of a proposition, or of a paragraph, there is usually a red letter and a new line; but frequently also this distinction is made in the middle of a sentence, while the beginning of propositions and subjects in other places, is not distinguished in that or in any other manner. The enumeration of the propositions is often irregular; and many of the diagrams, though neatly drawn, are altogether erroneous. The MS. was sold at Dr Moor's death, and was afterwards purchased for the Library of the Faculty of Advocates at Edinburgh.

" This MS. not having the 7th book, Dr Moor procured a copy of it to be taken from the MS. No. 2368, in the Parisian Library. This, also, for some time, was in Dr Simson's possession, who in a note to Commandine's Pappus, gives the following account of the transcript, which he must have got from Dr Moor. Hunc autem librum 7^{um} Pappi ex eo codice (scil. No. 2368. Reg. Bibl. Par.) descripsit Dom. Caperonier, linguæ Græcæ in Academia Parisiensi Professor; sumptibus D. Jac. Moor, Collegæ mei doctissimi; Schemata vero ejus depinxit D. Jos. Brisbane, M. D......This copy, in the dispersion of Dr Moor's library, seems to have been lost."

A few years ago, the late Dr Charles Burney procured a manuscript, which had been the property of the Saibante Family at

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which is occupied by the six last books of Pappus. He states the part which precedes them, to [illegible]ntum arithmetici opusculi, ab incerto auctore scripti sine epigraphe. [illegible]ns sic; γαρ αυτους ελασσονας κ.τ.λ. which evidently shews, t[illegible] of the 2d book of Pappus, although he was not aware of it.

Verona. There is an account of it in Scip. Maffei Verona illustrata. He describes it as "Le collezioni di Pappo Alessandrino con le figure, nel fine si dicon' otto libro, ma i primi due mancano anche nel MS. Fu du Francesco Barocci nobil Veneto, e mathematico insigne, e fu da lui corretto tutto e accresciuto de varie lezioni ne margini, onde pare, che volesse publicar quest' autore, benche tralasciasse poi prevenutto forse del Commandino. Questo e forse l'unico codice, che in Italia rimanga dell' esimia racolta del Barocci qual da Venezia passo ad arrichir & Inghilterra." This last sentence is very curious, when connected with the actual fate of the book; and it is still more remarkable, that a large portion of the Greek MSS. from the Saibante collection, was purchased in 1820 by the University of Oxford, which was before in possession of so many of the Codices Barociani. The Abbe Celote, however, had previously sold this to Dr Burney, who esteemed it so highly, that when he had it new bound (for it is now in modern blue Turkey,) he had the words "codex prætiosissimus" impressed on the back of it. There are a number of blank leaves bound up at the beginning and end of the volume. On the first leaf of it, there is a Greek inscription, in a different hand from any writing in the body of the work; it begins with a quotation from S. Jerom, and seems to have nothing to do with Pappus. The water-mark on the blank leaves is the same as that of the paper on which the latter part of the 8th book is written; but the old paging is still very visible, which begins with the first page of the text of Pappus.

This manuscript, with the rest of the valuable library, which was purchased from the family of Dr Burney, is now in the British Museum. It either was originally written by several transcribers, or, which seems from some circumstances more probable, is made up of three fragments collected from different quarters. Pages 39,–48. (of the recent paging) contain the latter part of the second book, beginning with the same words as Sav. No. 9. This part appears to me to have been written by the same hand as the Sav. No. 3., and is exactly what is wanted in that manuscript. It is fair to add, that I pointed out this circumstance to a friend, who is much more familiar with manuscripts than I am, and who, while he admitted the similarity, was not convinced of the identity of the handwriting: but to me they appeared to be the same; and, from having recently collated the whole of No. 3., I was very familiar with the characters in which it is written. The final letters are frequently extended in the same forms in both manuscripts, and there is a particular mark at the end of the paragraphs, which is made in the same manner in both of them; the letters are of the same size; there is the same number of lines (30) in each page; and the length of the lines, as well as the intervals between them, is the same in both. Pages 49, 253, containing books iii, iv, v, vi, and the beginning of vii, are by a second hand. A small part then follows, in a third hand, at the bottom of page 253, and top of page 254; but this is loosely written, and evidently is an insertion of what was wanted between the second and last parts, for it does not extend to the bottom of page 254; and page 255, to page 481, contain the latter part of book vii, and the whole of book viii. These are written in a fourth hand, apparently older than the rest; and from page 487, to page 514, is a collection of diagrams, but they extend no farther than the 45th proposition of the 4th book.

The whole is on paper in folio, and in good preservation, although the red ink, in some places, is faded. There is a curious memorandum at the end of the 8th book:

> Ουτος ο πονος τȣ διορθωσαι ταυτα τα οκτω βιβλια των συναγωγων Παππȣ τȣ Αλεξανδρεως τετελειωται υπ᾽ εμου Φραγγισκȣ τȣ Βαροκιȣ ετι εκ της ημων σωτηριας α̅φ̅π̅η̅ τελευταιᾳ μηνος μαρτιου ημερα εν τη των Ενετων πανευδαιμονι πολει.

> Lucubratio hæc corrigendi hosce octo libros collectionum Pappi Alexandrini completa est a me Francisco Barocio Anno salutis nostræ MDLXXXVIII prid. kal. Aprilis in Venetorum felicissima civitate.

This is written in a different hand from any in the text of the work, so that it does not appear that any part of the copy was made by f; and how old it was when he employed himself i d correcting it, is hardly now to be ascertained. le that he talks of "hosce octo libros," while Maffei says, "si dicon otto libri, ma i primi duo mancuno," and that both should be inaccurate; but this only adds to the many instances which we constantly meet with, of the haste with which the accounts of books are drawn up. In the same manner, the Greek manuscript, No. 2368. of the French Kings' library, is said in the large catalogue to contain the eight books of Pappus; and it is added, "Primi initium desideratur," when, in fact, the whole of the first, and the beginning of the second, are wanting; the first words being γαρ αυτους ελασσονας, as in the Savil. No. 9.

Baroci seems to have studied our author with particular attention. Among the Latin MSS. of the Paris library, No. 7222, contains a translation of Pappus, "cum commentariis Francisci Barocii, præmittitur opusculum Italico idiomate scriptum, cujus is est titulus: Imperfettioni digli libri di Pappo, tradotto e commentati dal Commandino. Is codex decimo sexto sæculo exaratus videtur." Whether the prefatory dissertation is written by Baroci, or not, cannot be collected with certainty from this description; but the language in which it is written, and the age of the manuscript, are both in favour of his being the author; and its connection with the Saibante MS. makes it particularly curious. With this (the Saibante) great pains have been taken. It is corrected throughout; and there are a great many remarks and emendations written in the margins.

This manuscript contains, in common with those at Oxford, the passages pointed out by Dr Trail as wanting in Commandine's translation, at the end of the third and fourth books. They all, likewise, contain a lemma at the end of the seventh book, which is omitted in the translation. In fact, there is a remarkable similarity in the original text of Dr Burney's MS. with that of No. 3. of the Savilian library. Dr Burney's has the advantage of the text being, in some instances, complete, where there are lacunæ in the other; but the same false readings are generally found exactly in the same places; the same corrections are repeatedly inserted with ισως in the margins; and they both, as Dr Trail remarks of the Edinburgh MS., have the initial red letters sometimes inserted in the middle of a proposition. There are no diagrams inserted in the text of either, excepting for the eighth book; and, although these do not exactly agree, any more than the figures at the end of the two manuscripts, yet there is one place in the fourth book, in which there is a very remarkable coincidence;—a blank space is left in the side of the text of No. 3. for a figure; and this is the only instance in which any thing of the kind occurs throughout the volume; and exactly at the same place, a similar blank is left in the text of Dr Burney's manuscript. All these points of coincidence cannot be accidental, neither can they be accounted for by a conjecture, like that of Dr Wallis's, which would suppose that one is a transcript of the other. The transcriber of the Savilian MS. would not have left lacunæ in his text, where it was complete in his original; and the state of the diagrams, with their partial variations, proves that Dr Burney's MS., (even if it did not appear, at least in the latter part, to be of an earlier date,) could not be taken from the Savilian, No. 3. The whole, indeed, admits of a much more probable explanation, which will extend likewise to No. 9., and account clearly for its similarity to No. 3. It is well known, that the early manuscripts of many works are very few in number. In some cases, only one is known, from which the rest have been derived; and, in other cases, even that one has been lost. Is it not very probable that this may be the truth with respect to the text of Pappus? None of the MSS. of which we have any details, are of a very early date, and there appears to be a similarity in all, which have been examined. This would be the immediate consequence of their having all the same prototype, without leading, by any necessity, to the conclusion of one's being a mere transcript of the other. Several, no doubt, are, and must of course be, secondary copies, and that of modern MSS.; but this must be proved by other circumstances than those which lead Dr Wallis (as it should seem) too hastily to his conclusion.

The hypothesis now offered, may be considered as satisfactory with respect to the English manuscripts: how far it may be generally applicable to those on the Continent, can only be determined by a particular examination of each. Most catalogues give nothing but the titles of the books, or a very few particulars, from w[illegible] little can be collected; and of this little, the accuracy is n[illegible]e depended on. On this head, however, we may lo[illegible]rmation. Professor Excelmans at Paris is said [illegible]nsiderable advances in preparing the original Greek of Pappus for the press. The great difficulty of forming any thing like a tolerable text, must, of course, have led to a critical examination of different MSS.; and the apparatus with which this will have provided him, will add greatly to the value of a publication which has long been looked to with earnest wishes, but with little expectation.

ART. VI.—*Catalogue of the Declination of 36 Fixed Stars for 1820, according to the Observations of* Professor BESSEL. Communicated by Professor TRALLES of Berlin.

Declination of 36 Stars for January 1. 1820*, *as determined by the Observations of Professor* BESSEL *at Konigsberg, with the Differences between them and those in Mr* POND'S *Catalogue in the Nautical Almanack for* 1824.

Names of Stars.	North Declin.	Probable Error.	Pond.
	° ′ ″	″	″
α Aurigæ,	45 48 9.12	0.18	— 0.12
α Cygni,	44 38 28.47	0.18	+ 1.53
α Lyræ,	38 37 17.77	0.24	+ 2.23
α Gemini (seq.)	32 16 21.05	0.23	— 0.05
β Gemini.	28 27 5.54	0.22	— 0.54
β Tauri,	28 26 40.40	0.23	+ 0.60
α Andromedæ,	28 5 46.59	0.22	+ 0.41
α Coronæ,	27 19 34.44	0.22	+ 2.56
α Arietis,	22 36 22.32	0.23	+ 0.68
α Bootes,	20 7 25.43	0.21	+ 1.57
α Tauri,	16 8 17.16	0.22	— 0.16
β Leonis,	15 34 40.04	0.24	+ 1.96
α Hercules,	14 36 10.45	0.24	+ 2.55
α Pegasi,	14 14 19.05	0.24	+ 1.95
γ Pegasi.	14 10 56.22	0.23	+ 3.78
α Leonis,	12 50 33.58	0.22	+ 2.42
α Ophiuchi,	12 41 55.66	0.24	+ 2.34
γ Aquilæ,	10 10 53.97	0.23	+ 4.03
α Aquilæ.	8 24 0.69	0.21	+ 2.31
α Orionis,	7 21 50.69	0.22	+ 1.31
α Serpentis,	6 59 54 84	0.20	+ 2.16
β Aquilæ,	5 57 50.84	0.23	+ 5.16
α Canis min.	5 40 40.32	0.21	+ 0.68
α Ceti,	3 22 37.67	0.24	+ 4.33
β Virginis,	2 46 42.81	0.29	+ 2.19

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* See this *Journal*, Vol. I. p. 320.

	South Declin.			Probable Error.	Pond.
	°	′	″	″	″
α Aquarii,	1	11	25.48	0.23	+ 4.48
α Hydræ,	7	53	1.68	0.23	+ 4.68
β Orionis,	8	25	4.22	0.24	+ 4.22
α Virginis,	10	13	7.69	0.22	+ 4.69
1 α Capricorni,	13	3	25.59	0.35	+ 4.59
2 α Capricorn.	13	5	43.49	0.35	+ 6.49
1 α Libræ,	15	14	33.27	0.25	+ 7.27
2 α Libræ.	15	17	15.05	0.25	+ 5.05
α Canis maj.	16	28	37 15	0.23	+ 1.15
α Scorpii,	26	1	23.00	0.26	+ 4.00
α Piscis aust.	30	34	68.68	0.37	+ 2.68

It is to be remarked, that these observations have been made by a meridian circle of three feet diameter, of the construction of Mr Reichenbach at Munich. The errors of the divisions, though extremely small, have yet been determined by a particular method, after the circle had been put up in its place. The effect of the bending of the telescope five feet in length, though counteracted by levers, has also been taken into consideration. The circle can be inverted in its supports, so as to face either the east or the west. The Pole star can be observed very well in the day time in a bason of water three feet in length. The observations have been reduced by Mr Bessel's own table for refraction, which has been improved since the publication of his *Fundamenta Astronomiæ*.

ART. VII.—*Abstract of a Paper on the Electrical Phenomena produced* in Vacuo [*]. By Sir HUMPHRY DAVY, Bart. P. R. S. &c. &c.

THE object of the distinguished author of the paper of which we propose at present to give an abstract, was to determine *the relation of electricity to space, as nearly void of matter as it can be made on the surface of the earth.* Mr Walsh had maintained, that the electric light could not be produced in a perfect Toricellian vacuum, and Mr Morgan had concluded, that such a vacuum prevented the charging of coated glass.

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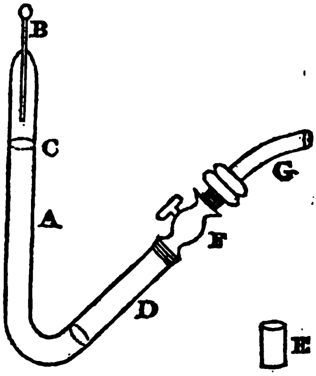
[*] The pap abstract is published in the *Phil. Trans.* 1822, Part I. p. 64.

The existence of mercurial vapour in the most complete toricellian vacuum, induced Sir Humphry Davy to doubt the accuracy of these deductions, and he was therefore led to put them to the test of experiment, by forming the vacuum with a comparatively fixed metal in fusion.



The apparatus which he used for this purpose, consists of a curved glass tube AD, having one of its legs A closed, and longer than the other. In the closed leg CA, a wire of platinum B was hermetically cemented, in order to transmit the electricity; and for the purpose of ascertaining the power of the vacuum to receive a charge, a small cylinder of metallic foil E was placed as a cap, on tubes that had not the wire B. When the closed leg had been filled with mercury, or fused tin, whose surface stood at C, the open end D was exhausted by the stopcock F, connected by the moveable tube G, with an excellent air-pump, and to ensure greater accuracy, the exhaustion was made after the tube and apparatus were filled with hydrogen by means of the stop-cock F.

By the aid of this apparatus, in which the rarified air or gas could be made to balance a column of fluid metal of any length from $\frac{1}{20}$th of an inch to 20 inches, Sir Humphry Davy could easily procure a vacuum either of a large or a small size; and by using recently distilled quicksilver, and boiling it in vacuo, six or seven times, from the top to the bottom, and from the bottom to the top of the tube, and making it vibrate repeatedly, by striking it with a small piece of wood, he obtained a column in the tube free from the smallest particle of air.

The vacuum being thus rendered perfect, Sir Humphry found that in every case the mercurial vacuum was permeable to electricity, and could be rendered luminous either by the common spark, or by the shock from a Leyden jar, the coated glass becoming charged. The intensity, however, of these effects, was found to depend on the temperature. When the tube was very hot, the electric light appeared in the vapour of a *bright green colour*, and of great density; but it became less vivid as the temperature diminished, and when it was cooled down to 20° below the zero of Fahrenheit's scale, it was so faint that it could only be seen when the darkness was considerable. The charge communicated to the tin or platinum foil F, increased also with the temperature, and became extremely feeble at 0° of Fahrenheit.

In boiling the mercury in the exhausted tube, Sir Humphry observed a very beautiful phenomenon. In the formation and condensation of the globules of the pure and dense mercurial vapour, which was thus generated, the electricity produced by the friction of the mercury against the glass, was discharged through the vapour, in sparks so extremely brilliant, that they were visible in day-light.

By introducing the minutest quantity of rare air into the mercurial vacuum, the colour of the electric light changed from *green* to *sea-green*, and by increasing the quantity, to *blue* and *purple*; and when the temperature was low, the vacuum became a much better conductor.

Sir Humphry Davy next proceeded to form a vacuum by means of fused tin. By using freshly cut pieces of grain tin, and melting them in a tube made void after being filled with hydrogen, and by applying long continued heat and agitation, he obtained a column of fused tin entirely free of gas. In the vacuum thus formed, the same electrical phenomena were produced as in the mercurial vacuum, at a temperature below 0°. The electric light was *yellow*, and of the palest phosphorescent kind, requiring almost absolute darkness to be perceived; and it was not perceptibly increased by heat *.

In some of the first of these experiments, Sir Humphry Davy connected the metal with the stop-cock by means of a wire; but afterwards the rarified air or gas was the only chain of communication, and he was enabled by that circumstance to ascertain, that the feebleness of the light in the more perfect vacuum was not *solely* owing to a smaller quantity of electricity passing through it; for the same discharge which produced a faint *green* light in the upper part of the tube, produced a bright *purple* light in the lower part, and a strong spark in the atmosphere.

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* Sir Hum [illegible] d, that electrical and magnetical attractions and repulsions took [illegible] have done in air.

In order to try the effects of other vapours, Sir Humphry formed a vacuum with pure *olive oil,* whose boiling point is not much below that of mercury, and also with the *butter,* or *chloride of antimony,* whose boiling point is about 388° of Fahrenheit. The electricity which passed through the vapour of the chloride was much more brilliant than when it passed through the vapour of the oil, and more brilliant through the vapour of the oil than through the vapour of the mercury, at common temperatures. The light was *purely white* in the vapour of the *chloride,* and *red,* approaching to *purple,* in that of the *oil;* and, in both cases, a permanent elastic fluid was produced by its transmission.

Considering the diminution of the density of vapours, by diminution of temperature, as in a geometrical progression, while the decrements of temperature are in an arithmetical progression (a law which appears to be established by the experiments both of Mr Dalton and of Sir Humphry Davy), and considering the boiling points of

Mercury,	as at 600° of Fahrenheit.
Oil, . .	540
Chloride of Antimony,	340
Tin, . .	5000

ll above 52°, and taking the elastic force of the vapour of water t this temperature, to be such as to balance about 45 parts of n inch of mercury; Sir Humphry Davy finds that the relative trength of vapour will be, for *mercury,* 000015615; for *olive il,* 0016819; for *chloride of antimony,* 01692; and for *tin,* 37015, preceded by 48 cyphers.

Sir H. Davy next endeavoured to ascertain if the power of he Toricellian vacuum to transmit electricity, was diminished by diminution of temperature. This appeared to be the case to bout 20° above zero; but between + 20° and — 20°, the lowest emperature that he could produce by pounded ice and muriate f lime, it seemed stationary. The light was in this case visible during the transmission, unless the electrical machine was very active, in which case there was a pale phosphorescent light above, and a spark in the mercury below, and a brilliant light in the common vacuum. A Leyden jar, *weekly* charged, was ample to transmit its electricity, by explosion, through the cooled Toricellian vacuum, but the electricity was slowly dissipated through it. When the jar, however, was strongly charged, the spark passed through nearly as much space as in common air; and, with a light visible in the shade, Sir Humphry found also, that the mercurial vacuum was a much worse conductor than highly rarified air, at all temperatures below 200°; and when the tube which contained it was included in the exhausted receiver, its temperature being about 50°, the spark passed through a distance *six* times greater in the Boylean than in the mercurial vacuum.

From these very interesting results, Sir Humphry concludes, that light, and probably the heat generated in electrical discharges, depends *principally* on some properties or substances belonging to the ponderable matter through which it passes; and also that space, containing no appreciable quantity of this matter, is capable of exhibiting electrical phenomena *.

ART. VIII.—*An Account of a Map of the Vicinity of Paukgan, or Pagan.* By FRANCIS HAMILTON, M. D., F. R. S. & F. A. S. Lond. & Edin., &c. Communicated by the Author.

THE accompanying Map, Plate IV., was constructed by the town-clerk (Mro Za-re) of Paukgan, during a short stop I made there, on the return of the embassy from the court of

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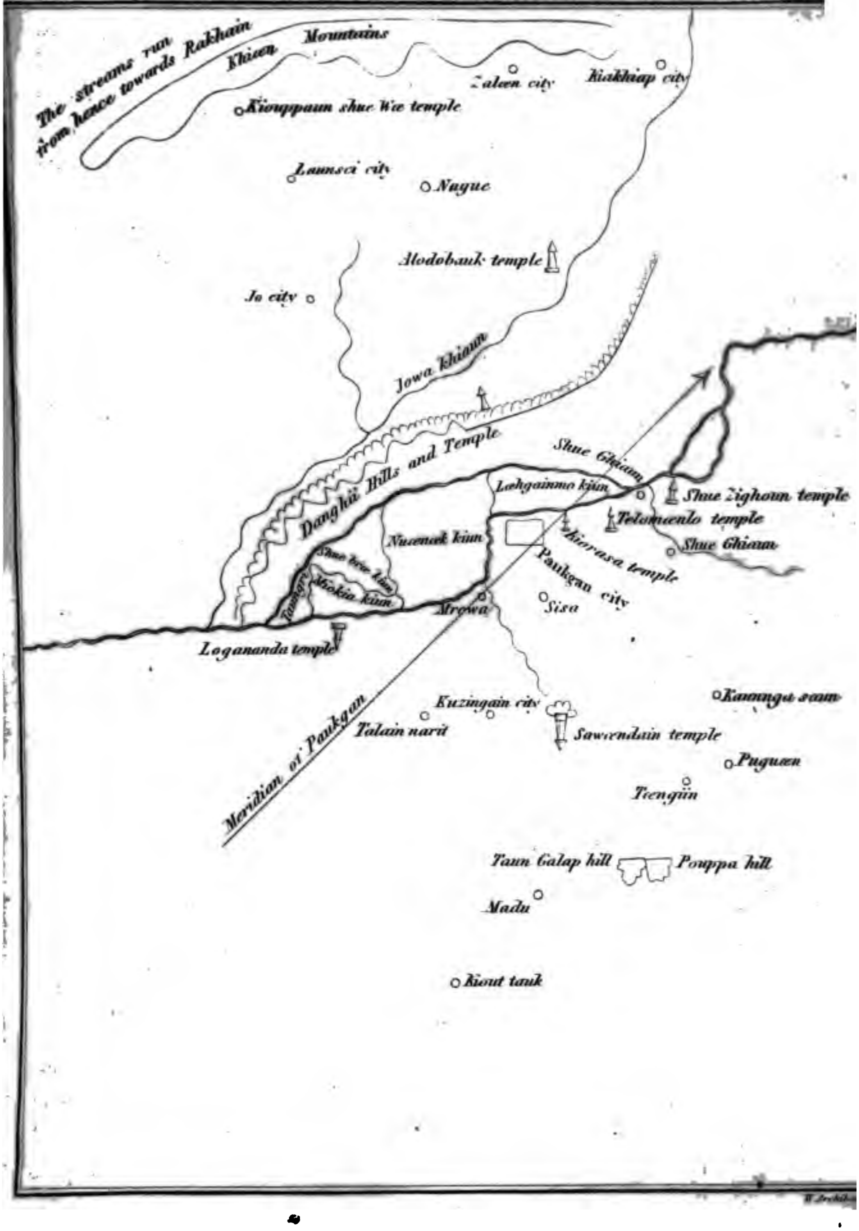
* In the course of his experiments, Sir Humphry Davy found, that recently distilled mercury, which has been afterwards boiled and cooled in the atmosphere, emits air when heated in vacuo, and that the lower stratum in a barometer tube had imbibed air. Hence he considers it very probable, that air exists in mercury, in the san [illegible] tate as in water, that is, distributed through its pores; and, consequen [illegible] n barometer and thermometer tubes, should be long boiled, an [illegible] oiled after a certain lapse of time. Sir Humphry also think [illegible] tains air.

PLATE IV



Published by A Constable & C^o Edin^r 1822.

Ava. This person seemed to be under no apprehension of giving information, and was an intelligent obliging man, like his master, the chief officer of the city (Mrosagri), who was said to hold his office by hereditary right, and to be by birth a Talain or Peguer, descended of the ancient princes of that nation, who at one time governed the empire as sovereigns. During my short stay, the clerk was improving fast in understanding the nature of our maps, and was engaged in drawing the one here given, when our sudden departure put a stop to his work in a very imperfect state. I have, however, given it here on a reduced scale, as it is the only authority that it is yet known, for the course of the Jowa river (khiaun), the country watered by which is a blank, even in the map of Asia by Mr Arrowsmith.

The first intelligence that I received concerning the country watered by the Jowa, was from the town-clerk (Mro Za-re) of Rangoun, who conducted the boats of the embassy. On our way to court, being nearly opposite to its mouth, he told me, that two days' journey up this river there is a large town, called Jo. In its neighbourhood are many villages, inhabited by a people of the same name, who, he said, are very ugly, having white teeth, large bellies, and long loose hair. White teeth, it must be observed, are considered by the Mranmas as too like those of dogs; and therefore, all fashionable people in this empire are at great pains to dye theirs black. The Jo, the clerk said, speak a dialect of the Mranma language, differing from that of the court, in an uncouth provincial accent, which information I found to be correct. They possess a rich country, where they raise much sugar-cane, of which they inspissate the juice, boiling it into the substance, which, by Europeans, in India, is called *Jagori.*

The next mention that I heard of the Jo country was from an officer of the guard appointed at Amarapura to attend the ambassador. He said, that the country of the Jo occupied the space west from the lower part of the Khiænduæn river, commencing at a little distance from its bank, the immediate vicinity being inhabited by Mranmas. The Jowa, on the banks of which the Jo live, must therefore run from the north to the south, as its mouth is at some distance south from that of the Khiænduæn, and the courses of the two are parallel. North from the

Jo, this man said, between it and the Kasi Shan, is a mountainous and woody tract reaching to near the Khiænduæn, and occupied by the rude tribe called Khiæn.

About the same time, I received from the slave of the heir-apparent the general map, published in the second volume of this Journal, where the country of the Jo is laid down as extending from opposite to Pagan as far north as the proper country of the Mranmas, and separated from the latter, and from the banks of the Khiænduæn, by a range of hills, which are no doubt the Danghii mountains, mentioned (page 263.) in my account of that map; and more fully described in Vol. IV. page 82. of this Journal, where several errors in this part of the slave's map are pointed out. Although the slave in his map lays down in the territory of the Jo only four towns dignified with the title Mro, yet he mentioned orally, that this tribe was governed by six hereditary chiefs, with the title of Zabua, namely, Zho, Kakhiap, Thilæn, Jo, Launsci, and Taunduæn; but in the memoir, which I have published in the fourth volume of this Journal (page 83.), I have stated it as probable, that Taunduæn does not, in fact, belong to the Jo, but to the tribe named Ængün, which will greatly curtail the extent of territory given to the Jo in the general map of the slave; for there it is made to extend a distance of about 100 German miles, (Phil. Journ. Vol. VI. page 110.) From this we may probably deduct 30 miles for the territory of the Ængün, leaving a large space between the territory depending on Taunduæn and the southern frontier of the Kasi Shan; and this is occupied by the mountains of the Khiæn, as mentioned by the officer on duty with the embassy. The extent of territory, therefore, belonging to the Jo is perhaps about 50 German miles from north to south; for its southern boundary reaches to the territory of Zhænbrugiun, a town about 18 or 20 miles SSW. from Paukgan in a straight line.

Zhænbrugiun (Island of White Elephant) is the frontier government of Ava towards Arakan, extending from the Erawadi to the great mountains of the Khiæn, which separate the two kingd [illegible] chief town of this government, named also Zhænbru [illegible] rainy season stands on the west bank of the great [illegible] island before it. At that time the channel between the island and Zhænbrugiun, through which I passed, admits the largest boats; but, on the return of the embassy, on the 6th of November, I landed on the east side of the island, at a village called Læbænzeip (Four-tree Port), which serves as the port to Zhænbrugiun, the channel between the town and island being then nearly dry. From Læbænzeip I saw clearly the hills of the Khiæn west from me, as I guessed at 50 or 60 miles, called three days' journey, and of considerable height, perhaps 4000 or 5000 feet perpendicular. The people here pointed out the situation of Launsci, bearing about NNW., and said by them to be distant three days' journey. Here I farther learned, that none of the Jo tribe occupied the country farther south than nearly opposite to Paukgan. The low chain of hills immediately north from the mouth of the Jowa, which at Paukgan was called Danghii, was at Zhænbrugiun called Gnambændaun (Indian Fig-tree Hills). Between this chain and that separating Ava from Arakan, is a level fertile tract, occupied by the Jo nation, and extending 50 or 60 miles each way. There is reason, however, to believe, although such is the extent of the territory occupied by the Jo, that the plain extends considerably farther from north to south, comprehending not only the territory of the Jo, but also that of the Ængün, which may reach 30 miles from north to south. In the latter direction, the territory of the Jo is not bounded by any hills; and in the government of Zhænbrugiun, the plain extends the whole way between the Erawadi and the mountains of the Khiæn. Some way farther south, below Maenbhu, the western banks of the Erawadi are hilly; but whether these hills extend to the Khiæn mountains, or whether the plain of the Ængün, Jo and Zhænbrugiun extends behind them, as it does behind Danghii, I cannot say. There are, however, several governments (Mro) in that space, which implies, that there is a considerable extent of cultivated land.

When I arrived at Paukgan, on the return of the embassy from Court, I first received a visit from a dependent of its hereditary chief, who manages five villages belonging to his principal, that are on the banks of the Jowa, at its lower part, but west from Danghii. This dependent's house was at Warabiæn, from which the mountains of the Khiæn are 20 leagues (Dain) or 44 miles distant. Launsci, the nearest Jo town, is but a small way from Warabiæn, and, besides Jo, its territory contains many Khiæn. Thilæn is the next town of the Jo, and is one day's journey north from Warabiæn. Beyond this, he knew Jo, Zho, and Kakhiap, as districts occupied by Jo.

Soon afterwards I received a visit from the hereditary Chief of Paukgan, with whom were several good looking young men, his relations. They said that Thilæn is directly opposite to Paukgan; above this are the five villages dependent on Paukgan, and above these Jo Mro. They also said that Zho, Jo and Thilæn are very large governments, under the authority of Zabuas, or hereditary princes; while Launsci was governed by a Mrosagri or Lieutenant on part of the lady, who was mother to the Prince of Paukgan.

In the evening I accompanied Captain Symes to return the visit of the hereditary Chief, and at his house became acquainted with the person who composed this map, and who accompanied me to my boat, where we began our geographical investigations, by his looking at some European maps, and having their nature explained to him. Next morning he returned with an old Jo man. The accounts which these two men gave me of the country opposite to Paukgan was as follows: The Jowa river, from its mouth upwards, has a northerly course for a considerable way, between Danghii and the Khiæn mountains. It then bends towards the west, rising from the mountains bordering on Arkan. The lower part of this small river has both its banks inhabited by Mranmas, subject to Paukgan, and occupying two clusters of nine villages each; the term *Rua*, which I have translated village, implying not only the cluster of houses, but the territory cultivated by the inhabitants. Beyond these villages, but on the east side of the Khiæn mountains, is Launsci, a town of the Jo, ten leagues or twenty-two miles from Danghii, meaning, I presume, the temple, eight leagues north from Launsci is Zho, and eight leagues farther is Jo, otherwise called Kakhiap, and still four leagues beyond this is Thilæn. Between th [illegible] he Jowa turns to the west, where it springs from the [illegible] ich separate Jo from Arakan. Above Kakhiap [illegible] Kaleh and Saundut, two towns of the Shan. It [illegible] d, that this account entirely reverses the position of the Jo towns, as given by the slave, making Thilæn the one farthest north, in place of being the one farthest south, as the slave represents; and it must also be observed, that the authority of the slave is supported by that of the hereditary chief's kinsman, who placed Thilæn directly west from Paukgan, as already mentioned. Neither is the account of the town-clerk confirmed by the map which he was drawing, when stopped by our sudden departure, while it was unfinished. Many of the differences, indeed, may be owing to this circumstance, as he would probably have altered the names and situations of the places, when he came to review and complete his work; for I probably mistook his meaning, while I wrote the names during the operation. Thus, perhaps, I wrote Jo in place of Zho, the words being pronounced nearly as if written Yo and Jo in English. Thilæn, it must be observed, is not laid down in this draught. Whether he intended to place it on the Jowa opposite to Kakhiap or Jo I cannot say. This would have been agreeable to what he told me on the day before; but I suspect that I then mistook his meaning, as all other authorities agreed in placing Thilæn in the southern part of the Jo territory. The general course of the Erawadi being from north to south, we are to consider the term above as signifying north, and below as signifying south; but the map is drawn very obliquely, the cardinal points being near its corners; for the remarkable hill called Pouppa bears about E.S.E. from Paukgan, and, therefore, the meridian will be nearly in the line that I have added.

On the whole, rejecting Taundusæn as a city belonging to the Ænghün and Launsci, which, although partly occupied by Jo, yet seems to be a dowery settled on one of the king's wives, we have Jo, Zho, Thilæn, and perhaps Kakhiap as capitals of the petty princes of the Jo nation, each probably possessing a territory like one of our shires or counties, that is to say, from 600 to 800 square miles in extent.

About the middle of the sixteenth century of the Christian era, Paukgan is reported by the natives to have been the seat of empire; and considerable portions of the wall and ditch still remain, from which we may judge that its fort has been fully as large as that of Amarapura. There is now much empty space within the walls, and the houses are uncommonly mean; for the greater part of the commerce and manufactures of this government are carried on at Gnaunu, a large town on the bank of the river above Paukgan, immediately opposite to the uppermost island, laid down in this map. The city of Paukgan, when it was the seat of empire, was called Arimattana; and from the vast number of temples both within and without the walls, to the distance of two or three miles all around, it must have been a splendid place. Many of these ancient temples are hollow within, and arched above; but, at present, it is said the art of constructing arches has been totally lost throughout the empire, so that the people seem in this point to be retrograde. Most of the temples, and places of residence for the priesthood, which were also built of brick and stone, are ruinous; but several of them, when we visited the place, had lately undergone, or were undergoing, a thorough repair, at the expence of the heir-apparent,—from which many people conjectured that this prince intended to make Paukgan his residence, should he become king. His death before that of his father has put a stop to this project; nor have I heard where his son, who now reigns, has fixed his residence.

During our journeys up and down the Erawadi, we visited three of these temples, favoured by the Prince. Logananda, towards the lowest extremity of this map, stands close by the river at the termination, in that direction, of what may be considered as the ruins of Arimattana. It is placed on a semicircular area of considerable elevation, to which there is an ascent on three sides by stairs, covered with wooden galleries, highly ornamented; and, although not to be compared with Shue Modo at Pegu, nor Shue Dagoun at Rangoun, is a large solid building. Its base, which is octagonal, diminishes by four terraces, to the two lower of which there is an ascent by three stairs opposite to those leading up to the area. This part of the building has lately been painted white, and the upper part above the terraces, which is in form of a cupola, had been uniformly and [illegible] gilded, which gave it a splendid appearance, notwithst[illegible]siness.

The te[illegible]nda, about half a mile east from the city wall, [illegible]f the finest buildings in the empire;

and, when we saw it, it was under repair. The centre of the building is quadrangular, and its roof diminishes to a point by seven terraces, in imitation of the seven stages of the mountain, which, in the ideas of the Mranmas, occupies the centre of the universe. On two sides the centre has a narrow wing, forming with the centre a kind of cross. The interior, unlike most of the temples we have seen in the empire, is hollow, and subdivided into narrow galleries with arched roofs. The gates and windows also, which are numerous, are all arched, and highly ornamented.

Shue Zighoun, at the upper boundary of this map, resembles much Logananda in structure, but is larger, and I should guess not less than 150 feet in perpendicular height; but the area on which it stands is not elevated, and it is surrounded by more small structures, images and ornaments. The wooden galleries, which, as usual, surround the area, are not only very elegantly carved and gilded, but are within adorned with paintings of flowers and men in very bright colours, although of rude execution. Here, as well as in other temples near Paukgan, we observed figures of Gautama lying and standing, as well as sitting, which in other parts is the usual posture. This temple had been repaired a considerable time longer than Logananda; so that the gilding was in many places defaced, and its appearance was not so splendid, although, on the whole, it is a finer work, surrounded by stately trees, paved walks, and neat convents for priests, who do not seem, however, attached in any manner to the places of worship. Temples of this structure, ending in a cupola instead of a spire, as the great ones in Pegu generally do, are pretty common in this part of the country, as the very celebrated temple called Kaunhmudo, on the opposite side of the river, some way higher up, is of the same structure. They are also common with the followers of Buddha in Nepal, both Swayambhunath and Kasacheit having this form. Those like Ananda having a cavity within, and consisting of a quadrangle surmounted by a pyramid of seven stages, seem nearly peculiar to the vicinity of Ava. There are many such at Paukgan; and the temples at the angles of Amarapura are similar, only the terraces are not supported by arches, but upon wooden beams. The most ancient temple, however, of Buddha in India, and, perhaps, in the world, namely, that at Budda Gya in Magadha, seems to have been somewhat on the plan of Ananda.

Among the ruins of Paukgan, as I have said, are many buildings constructed of brick and stone, that are said to have been the abode of a priesthood; but compared with the elegant and airy wooden convents now in use, they have been wretched abodes.

The immediate vicinity of Paukgan is uncommonly sterile, and consists of swelling lands, here and there rising into little hills in broken ridges. The soil, except in a few narrow bottoms, is every where of a harsh sand, consisting of quartz and mica, with some small rounded pebbles intermixed. The rock, in many places, comes to the surface, and consists of the same materials which form the basis of the soil; but I must leave it to geologists to determine, whether the soil proceeds from the rock decomposed, or whether the rock arises from the soil concreted. In some parts I observed vertical dikes of a more compact nature, running parallel to each other through the common mass of gravel and rock, and often crossed at nearly right angles by other dikes of a similar nature. These dikes generally rise higher than the sandstone which fills the interstices between them, and thus become more conspicuous. This kind of country extends along the east bank of the Erawadi, so far as we went inland, from Gnaunu to Lunghii, amounting to nearly 1° 40′ difference of latitude. The surface of all this space has scattered over it pieces of agate so strongly resembling wood, not only in general form, but in grain and layers, that I have little or no doubt of its being a petrifaction. Similar agates, found both in the Birbhum district of Bengal, and in the south of Hindusthan, are, by the natives of these countries, considered as petrified tamarind trees, to the wood of which they have a strong resemblance. In one place I observed fragments of these agates intermixed with the sand and pebbles, of which the rock consisted; and this rock also contained portions of a reddish ochre of iron. In another place a rock, composed as usual of sand and gravel, contai [illegible]: but whether of marine or fresh-water testaceous ani [illegible] nitted to notice. In general, however, the agat [illegible] tirely to the surface, where it lies quite loose; [illegible] iks did I ever observe it immersed at a depth of more than two or three feet. Its masses are sometimes three or four feet long, and eight or nine inches in diameter, but such are rare.

In the vicinity of Gnaunu I observed also lying on the surface, masses of the common sand of the soil, which had the form of branches of trees or corals; but, except the general cylindrical and branched form, these masses had nothing to indicate that they ever had been endowed with either vegetable or animal life, or owed their form to bodies of these natures.

This sandy territory, extending from Paukgan to Lunghii, has Renanghiaun and its petroleum wells in the centre, and there it is more broken, rocky and barren than any where else; but, except in narrow bottoms, winding among the sandy hills and swelling grounds, and these nowhere of considerable extent, it does not admit of the cultivation of rice. Nor, except in such bottoms, do trees grow to a considerable size. Near some convents, indeed, in such situations I observed many stately Mango and Tamarind trees, and the palm called Borassus in a most flourishing state; but the waste sandy lands are covered with stunted trees or bushes of the genera called Acacia, Cassia, Zizyphus and Cissus, and, besides, produce more pasture than I expected; for we saw many cattle, and these not in very bad condition. Where the soil, although sandy, is level enough for the plough, there is a good deal of cultivation, and the crops of various kinds of pulse, of maize, of Holcus sorghum of Crotolaria juncea, of Sesamum, and above all, of cotton, were much better than the appearance of the soil might seem to promise. Much cotton is reared here, and forms a considerable article of commerce with the Chinese province of Yunan.

Art. IX.—*Abstract of a Paper on the Anomalous Magnetic Action of Hot Iron, between the White and Blood-red Heat.* By Peter Barlow, Esq. of the Royal Military Academy.

In a preceding volume[*], we have already given a brief notice of the curious discoveries of Mr Barlow, respecting the magnetism of red-hot iron. The details of his experiments have since appeared in the Philosophical Transactions for 1822, p. 117., and are of such importance as to induce us to give a more full account of them to our readers.

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[*] See Volume vi. p. 360.

Mr Cavallo seems to have been the first person who found, that red-hot iron had a greater power over the magnet than cold iron. Captain Scoresby has more recently determined, that hot iron receives more magnetism of position than the same iron does when cold, a red hot-bar producing a deflection of 77°, whereas it was only 15° when cold *. In this state of the subject, Mr Barlow was led to investigate it, and obtained the very remarkable results which we have formerly explained.

The following Table contains the whole series of his experiments. The attraction which took place according to the known laws of magnetism is marked +, which ever end of the needle approached the iron, and the opposite attraction is marked —. When the compass, for example, is above the centre of the bar, the north end of the needle should be drawn towards the iron; but when the compass is below the centre, the south end should approach the iron. These, therefore, are both marked thus +, and the contrary attraction of the *red* heat is marked.—In the Table C. B. signifies *Cast-Iron Bar*, and M. B. *Malleable Iron Bar*.

All the following experiments were made with the bars inclined in the direction of the dipping-needle, and the *negative* attraction was always the *greatest*, where the *natural* attraction was the least, that is opposite to the middle of the bar, or in the plane of no attraction. Mr Barlow also tried a few experiments with the bar inclined at right angles to its former position; but though a certain quantity of negative attraction, never exceeding $2\frac{1}{2}$°, was always found, yet the results are not so strongly marked as in the other experiments.

The only possible explanation which Mr Barlow offers to account for these anomalies is, that the iron cooling faster towards its extremities than towards its centre, a part of the bar will become magnetic before the other part, and thus cause a different species of at[illegible] This explanation, however, he does not consider as [illegible]

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* [illegible]ons, vol. ix. part 1. p. 251.

Effect of Iron at different Degrees of Heat on the Compass.

No. of Exp.	Description of bar. 25 in. long, 1¼ in. square.	Height or depth of centre of bar from compass.	Distance of bar from compass.	Position of Compass.	Effect Cold.	Effect White Heat.	Effect Red Heat.	Effect Blood Red Heat.
		Inch.	Inch.		° ′	° ′	° ′	° ′
* 1	C. B. No. 1.	0.0	6.0	S. 80. W.	+ 0 0	0 0	— 17 0	0 0
2	M.B. No. 2.	4.5 below.	6.0	ditto.	+ 30 0	ditto.	0 0	+ 45 0
3	C. B. No. 2.	4.5 below.	6.0	ditto.	+ 18 0	ditto.	0 0	+ 49 0
4	M.B. No. 1.	ditto.	6.0	ditto.	+ 29 30	ditto.	— 12 0	+ 44 0
5	ditto.	13 below.	6.0	ditto.	Not obs.	ditto.	0 0	+ 52 0
6	ditto.	4.5 below.	6.0	N. 80 W.	ditto.	ditto.	— 12 30	+ 70 0
7	ditto.	4.5 above.	6.0	S. 80 W.	ditto.	ditto.	— 12 30	+ 30 0
8	ditto.	ditto.	6.0	ditto.	ditto.	ditto.	0 0	+ 25 0
9	ditto.	ditto.	6.0	ditto.	ditto.	ditto.	— 19 0	+ 30 0
10	ditto.	1.0 above.	6.0	ditto.	ditto.	ditto.	— 15 0	+ 4 0
11	M.B. No. 2.	12.5 below.	8.5	N. 80 W.	+ 29 30	ditto.	0 0	+ 37 30
12	ditto	ditto.	8.5	N. 80 E.	+ 30 0	ditto.	0 0	+ 41 0
13	C. B. No. 1.	12.5 below.	8.5	N. 80 W.	+ 16 0	ditto.	0 0	+ 42 30
14	ditto.	ditto.	8.5	N. 80 E.	+ 15 30	ditto.	0 0	+ 47 30
15	M.B. No. 2.	9.0 below.	8.5	N. 80 W.	+ 28 30	ditto.	— 1 0	+ 39 30
16	ditto.	ditto.	8.5	N. 80 E.	+ 29 30	ditto.	— 1 30	+ 42 0
17	C. B. No. 1.	9.0 below.	8.5	N. 80 W.	+ 15 45	ditto.	— 1 30	+ 45 0
18	ditto.	ditto.	8.5	N. 80 E.	+ 16 0	ditto.	— 1 30	49 0
19	M.B. No. 2.	6.0 below.	8.5	N. 80 W.	+ 25 0	ditto.	— 3 0	+ 32 30
20	ditto.	ditto.	8.5	N. 80 E.	+ 26 0	ditto.	— 3 30	+ 33 0
21	C. B. No. 1.	6.0 below.	8.5	N. 80 W.	+ 11 30	ditto.	— 3 30	+ 36 30
22	ditto.	ditto.	8.5	N. 80 E.	+ 13 0	ditto.	Not obs.	+ 36 30
23	M.B. No. 2.	3.0 below.	6.0	N. 80 E.	+ 8 0	ditto.	— 21 30	Not obs.
24	ditto.	ditto.	6.0	N. 45 W.	Not obs.	ditto.	— 25 30	+ 25 30
25	M.B. No. 1.	0.0	6.0	ditto.	0 0	ditto.	— 40 0	0 0
26	M.B. No. 2.	1.0 above.	5.3	N. 60 W.	+ 2 0	ditto.	— 4 30	+ 5 30
27	M.B. No. 1.	ditto.	5.3	ditto.	Not obs.	ditto.	— 12 30	+ 5 30
28	M.B. No. 2.	9.0 above.	6.0	N. 85 E.	+ 47 30	ditto.	— 2 30	+ 60 0
29	M.B. No. 1.	ditto.	6.0	ditto.	+ 47 30	ditto.	— 2 30	+ 60 0
30	M.B. No. 2.	1.0 below.	5.5	N. 45 W.	Not obs.	ditto.	— 55 0	+ 5 45
31	M.B. No. 1.	4.5 above.	7.0	N. 75 E.	ditto.	ditto.	— 2 30	+ 33 30
32	M.B. No. 2	1.7 below.	5.5	N. 45 W.	ditto.	ditto.	+ 100 0	+ 13 30
33	M.B. No. 1.	1.7 above.	5.5	ditto.	ditto.	ditto.	— 26 0	+ 13 30
34	M.B. No. 2.	1.7 above.	5.5	ditto.	ditto.	ditto.	+ 30 0	+ 13 30
35	M.B. No. 1.	4.5 above.	6.0	N. 55 E.	ditto.	ditto.	— 5 30	+ 35 30
36	M.B. No. 2.	ditto.	6.0	ditto.	ditto.	ditto.	— 0 0	+ 35 30
37	M.B. No. 1.	0.0	4.7	West.	+ 3 30	ditto.	— 50 0	+ 8 0
38	M.B. No. 2.	0.0	4.7	North.	0 0	ditto.	0 0	0 0

* No. 1. South end drawn to the bar at red heat.
No. 6. This bar being left standing, it attracted the same three days after.
No. 8. The needle suspected to touch the box.
Nos. 11. to 24. Observed at the same time with two compasses.
No. 25. N. end drawn to the bar at red heat. No. 26. Attractions very gradual.
No. 27. Passed suddenly to 12½", but returned immediately.
Nos. 28. and 29. Attractions gradual.
No. 30. Negative attraction rather sudden. No. 31. Motion of needle very slow.
No. 32. 100° very sudden, returned immediately. No. 33. Both attractions gradual.
No. 34. The same as No. 32; both anomalous. No. 35. Attractions very gradual.
No. 37. Motion regular, but quick. No. 38. No motion in the needle.

Art. X.—*On preserving Fish by Sugar.* In a Letter to Dr Brewster from J. MacCulloch, M. D. and F. R. S., &c. Communicated by the Author.

Dear Sir,

As you have not scorned to give your readers a useful notice respecting the preservation of meat by the use of vinegar, you may possibly not refuse to add to it some remarks on the curing of fish by means of sugar. This is a very antiseptic substance, as is well known; and though partially employed in hams, it is scarcely considered, as what it really is, one of the most active substances in their preservation, being rather employed blindly, and with the notion of giving flavour. It is highly deserving of a much more extended trial, particularly in the curing of meat for the Navy; as it does not, like salt, destroy the provisions, and as it is in itself nutritious.

But to return to the subject of this notice: Fish may be preserved in a dry state, and perfectly fresh, by means of sugar alone, and even with a very small quantity of it. I have thus kept salmon, whitings, and cod, for an indefinite time, and with the best effect; an experiment which I was led to try in voyaging among the Western Islands, where matters of this nature are often of considerable moment.

Fresh fish may thus be kept in that state for some days, (but I know not how long), so as to be as good when boiled as if just caught. If dried and kept free from mouldiness, there seems no limit to their preservation; and they are much better in this way than when salted. The sugar gives no disagreeable taste.

This process is particularly valuable in making what is called Kippered Salmon; and the fish preserved in this manner are far superior in quality and flavour to those which are salted or smoked. If desired, as much salt may be used as to give the taste that may be required; but this substance does not conduce to their preservation.

In the preparation, it is barely necessary to open the fish, and to apply the sug[illegible] muscular part, placing it in a horizontal position for [illegible] days, that this substance may penetrate. After th[illegible] dried; and it is only further necessary to wipe and ventilate it occasionally, to prevent mouldiness.

A table spoonful of brown sugar is sufficient in this manner, for a salmon of five or six pounds weight; and if salt is desired, a tea spoonful or more may be added. Saltpetre may be used instead, in the same proportion, if it is desired to make the kipper hard.

Trusting that I have taught you how to improve a Highland breakfast, I am, &c. J. MacCulloch.

Edinburgh, *July* 1822.

Art. XI.—*Account of the* Mémoires de la Société de Physique et d'Histoire Naturelle de Geneve.

THIS is the first part of a volume of natural and physical science, published by a society of "Savants" of Geneva. The society itself was constituted so far back as the year 1790, and many of the communications made to it, have, from time to time, been given to the public in different scientific journals, or in the particular works of their respective authors. By the present publication, it aspires to a more permanent character, and seems destined to take no mean station among the various philosophical societies and scientific institutions of Europe. Among its members we recognise many names already highly distinguished in the scientific world, and others which, though at present less known, will, we have no doubt, sustain the high reputation which their predecessors have acquired. Having already exhibited to our readers a list of the memoirs which form this portion of the *first* volume of the society's labours *, we shall proceed to notice a few of the principal papers.

In his "Memoir on the Fall of Leaves," M. Vaucher objects to the hypothesis that attributes the fall of the old leaf to the growth of the new bud; to that also which ascribes it to an alleged superabundance of juice in the plant, and defective transpiration; or to the inequality of growth between the circumference of the stem and the petiole of the leaf. The true cause, he supposes to consist in a peculiar structure that obtains at the insertion of the petiole with the stem. The fibres of the petiole, instead of being a simple prolongation of those of the stem, are, according to him, distinct from them. At the point where the separation of the leaf occurs, there is only a sort of approximation, or soldering, and not a real continuity of vessels. This soldering is supposed to be produced by the interposition of parenchyme between the two systems of vessels. As long as this parenchyme is supplied with juices, and retains its vegetative power, the adherence of the petiole is maintained; but when it begins to dry, the connection fails, and the leaf falls.

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* See this Volume, p. 193.

As this hypothesis is made to rest on *anatomical* grounds, we could have wished that M. Vaucher had exhibited something like a *demonstration* of the structure of these parts. He mentions, indeed, the existence of a circular ring or rising, visible exteriorly, at the place of junction of the petiole with the branch; but that this is caused by the interposition of cellular tissue between the vessels of the stem and petiole, or by any sort of soldering of the extremities of these vessels with one another, we have no evidence. We incline rather to the old opinion, that these vessels are truly continuous; and consequently, that whatever be the cause of the fall, it is accompanied by a rupture of vessels. That a diminution or cessation of vegetative power in the part precedes this event, has been generally deemed probable; though others, as M. Vrolick and Sir J. Smith, consider the fall of leaves in autumn as a sloughing of worn out parts, effected by the vital energy of the parts in contact with them.

In his memoir "sur les Charagnes," a species of *chara* that grows abundantly in the Lake of Geneva, and, from the use to which it is applied, called by the inhabitants "herbé à ecurer," M. Vaucher applies himself to discover its *fructification*, and correct the erroneous opinions of Linnæus, Schmidel, Hedwig, and Martin, with relation to it. To accomplish this object, he observed with care the germination of its seeds, which none of his predecessors had done. After many unsuccessful attempts, he was fortun[illegible]ing, in the month of November, what he deemed t[illegible] These he preserved through the winter, and in [illegible]pril following, had the satisfaction, in company with M. de Candolle, to see the commencement of germination in some of them, which at once decided their nature. He afterwards collected many other seeds from a marshy situation, in which they had been naturally sown in the preceding autumn, and caused them to germinate in the manner the former had done. At a period somewhat later, he also procured a great quantity of the young plants, which were easily detached from the mud in which they naturally grew, together with their seeds, rootlets and stems. From observations of this kind, he at length succeeded in rendering the history of their fructification nearly complete, and has given a description of the organs by which it is effected.

The plant grows in spring, from seeds shed in the preceding autumn; and about the beginning of June the rudiments of its flowers begin to show themselves at the place of the third or fourth whorl or verticil. The flower is composed of a globular stamen, which is sessile, of a fine cinnabar colour, and surrounded by a transparent membrane. Immediately above this membrane, the female flower is placed. It consists of a cylindrical capsule, of an elongated form, obliquely striated, and enveloped in a semi-transparent membrane, crowned by five or six prolongations, which M. Vaucher regards as stigmata. This corpuscle swells as it approaches maturity, and becomes ovoidal; the stigmata separate in the form of a disc with five deep lobes; the exterior membrane hardens and turns white, and the seed at length falls.

In the construction of the germ, we recognise an exterior envelope, formed of a mucilage, at first greenish, afterwards yellow, and, lastly, at the period of maturity, of a dirty-white hue. Underneath this mucilage is a second envelope, semitransparent, and of a horny consistence; it forms the only covering of the seed, and opens to give birth to the young plant at the period of its developement. The interior of the germ is filled with a mucilaginous or resinous matter, the particles of which are globular. These globules have been regarded as the true seminal particles; but they are only particles formed from a liquid substance, insoluble in water, with which it presents phenomena resembling those of oily mixtures.

The true seed of this plant, at the moment of its developement, occupies almost the entire capacity of the sac that contains it, through which it pushes forth its first filament. When this filament has reached a certain size, it begins to form its first verticil, which comes forth a little below the summit, exhibiting at first only one ray, afterwards two, and then three or more. In proportion as the filament elongates, we observe swellings to form below the first verticil, which give birth to simple radicles. These radicles prolong themselves, and serve to fix and perhaps nourish the plant. The seed remains long attached to the stem, so that the organs of the future fructification may be seen distinctly before the seed has finally separated. These several points in the fructification and germination of this plant, M. Vaucher has illustrated by suitable descriptive drawings.

This plant possesses some interest in a geological view. M. Leman is said by our author, to have proved, in a paper in the Journal des Mines, 1812, p. 341, that the small fossil, designated under the name *Gyrogonite*, and found in fresh water formations, is a petrifaction of different species of this aquatic plant. In truth, as he observes, the resemblance between the gyrogonite and the fruit of the charagne, is perfect. In farther proof of this opinion, Mr Vaucher remarks, that the exterior envelope before described, which at first is mucilaginous, becomes afterwards as hard as porcelain, so as to resist strongly any attempt to break it: and, in examining the bottoms of ditches, he has found great quantities of the seeds of these plants of great whiteness, and in perfect preservation. Such of these seeds as are too deeply buried to germinate, preserve themselves for a long time in this state; but at length they become penetrated by earthy matter, and formed into petrifactions. It would be interesting to ascertain if the substance of the gyrogonites be very different from that of the seeds of these plants. Our gyrogonites, adds M. Vaucher, are larger than the seeds of the common plant: but we also find gyrogonites much smaller; and there exist, moreover, some species of this genus, among others, tha[redacted] lake, *Chara tomentosa*, whose seeds are incomparably r n those which form the subject of this memoir.

The next memoir, on the Spermatic Animalcules of different animals, by MM. Prevost and Dumas, confirms the very singular facts discovered nearly a century and a half ago, by Leuwenhoeck and others, and more recently investigated by Spallanzani. The design of the authors is, to give a comparative description of these animalcules; and, more especially, to prove that they are the result of a real secretion. The rabbit, guinea-pig, hedgehog, cat, dog, horse, ram, goat, and mouse, were the animals selected from the mammalia. They were first killed by dividing the carotid artery, and an examination was then immediately made of the fluid contained in the testicles, and in other organs which communicate with the urethra, as the vesiculæ seminales, the prostate, and Cowper's glands. The three last organs do not exist in all animals, but the testicle is always present, and seems to be the only organ essential to the formation of the seminal secretion. On examining these several organs, the authors found the fluid of the testicle, and of its epididymis and vas deferens, to contain numerous animalcules, the various forms and sizes of which, in different animals, they have described and delineated in their memoir. Though animalcules were sometimes found in the vesiculæ seminales, it seemed entirely accidental, and they regard the testicle as the only secreting organ in which these minute beings are formed. The prostate, and the glands of Cowper, in particular, never contained animalcules. Whatsoever be the nature of these beings, their identity in each species of animal, their appearance at the developement, and through the active condition of the genital organs, and their absence in sterile males, seem to shew that they are the result of a true secretory function, and the active agents in all fecundation.

In birds, and in animals with cold blood, the genital organs consist only of the testicles, and their excretory ducts. In the seminal fluid of the common cock, the authors found an immense number of animalculæ, and in that also of the drake and sparrow.

Among reptiles, the viper, frog, and salamander were examined, and innumerable animalculæ were found in the seminal fluid of each. The seminal fluid of different species of snails and slugs afforded similar results.

The authors regret they cannot speak so decisively of the seminal animalcules of fishes. Spallanzani has described and delineated animated globules in the milt of fishes; and Haller speaks of animalcules furnished with a tail. The observations of the authors accord best with those of Spallanzani, but they are not sufficiently complete as yet, to warrant publication.

The authors, wishing to try the effects of certain agents on the irritability of these animalcules, exposed them to weak discharges of common electricity, by means of a Leyden phial, which soon destroyed their vital motion. Others were placed in a current of galvanic electricity, which produced no alteration in their movements; but, after a time, the animalcules, collected around the positive pole, were immoveable, while those at the opposite pole, were as lively as ever. This the authors found to be due to the action of the acid produced at the positive pole; and hence fluids weakly alkaline, as pure saliva, are most favourable to the continuance of vital movement in these animalcules. Other agents, as opium and prussic acid, can be employed only through solution in water; and, as pure water itself sometimes abolishes all vital movement, no accurate conclusion can be drawn as to the action of those substances on the irritability of these beings.

The authors conclude their interesting memoir with observing, first, That spermatic animalcules have nothing in common with infusory ones, except in their microscopic size; secondly, That they are produced in the testicles alone, but do not appear in those organs till the age of puberty; and, thirdly, That they appear to be the active principle or agent of the semen.

The last memoir of this portion of the volume exhibits the experiments of M. De Saussure, on the "Influence of Green Fruits on the Air, before the period of Maturity." It was called forth, by an essay of M. Berard, published in the sixteenth volume of the Annales de Chimie. In that essay, M. Berard is said to have arrived at the remarkable results, "That green fruits, through every period of their growth, do not comport themselves, like leaves in the sun; that they do not then decompose carbonic acid gas [illegible] ge oxygen; but that the only action which they ex[illegible] phere, in all periods of their vegetation, is to tra[illegible] n into carbonic acid." He is even led to believe, that, in equal times, green fruits consume more oxygen in sunshine than in the shade.

In his "Recherches Chimiques sur la Vegetation," M. De Saussure formerly maintained, in opposition to Ingenhousz, that green fruits, exposed in air to the sun, under certain circumstances, contributed to improve it; and it is the purpose of the present memoir to confirm and extend that opinion by more accurate experiments and details, and thereby to assimilate the action of green fruits upon the air to that which is produced by leaves. It is, however, said, that even Ingenhousz found, that if green fruits, as pears, grapes, and cucumbers, were immersed in spring-water, and then exposed to the agency of the solar rays, they often disengaged oxygen gas, like leaves; and this is a result which the experiments of M. De Saussure abundantly confirm.

Green peas (pisum sativum), both in the pod, and when shelled, were exposed to the sun, while immersed in spring-water, from 11 o'clock A. M. to 4 o'clock P. M. One hundred parts of the air of the water

Before the experiment, consisted of		*After* the experiment, 100 parts consisted of	
Oxygen, -	8.0	Oxygen,	38.25
Azote, -	16.5	Azote,	61.75
Carbonic Acid,	75.5		

A portion of the green leaves of peas, placed in the same circumstances for the same time, afforded an air,

100 parts of which consisted of 53.0 Oxygen.
47.0 Azote.

A similar quantity of the hollow stems of the same plant similarly treated, yielded an air, which, in 100 parts,

contained 38.0 Oxygen.
62.0 Azote.

These results shew, that the green pods and hollow stems of peas, when immersed in water impregnated with carbonic acid, and exposed to the sun, act, like leaves, in producing oxygen gas, but in an inferior degree.

When similar pods of peas were exposed in vessels of atmospheric air, inverted over mercury for twelve hours during the night, the air suffered a slight diminution of volume, lost about $\frac{1}{4}$th of its oxygen, and acquired a considerable portion of carbonic acid.

The experiment was next varied by placing the same quantity of peas in nearly an equal volume of atmospheric air, and inverting the vessel over water. The vase was then exposed to the direct action of the solar rays, moderated by being passed through a window. As a single day was not sufficient to afford a decisive result, the vegetables were withdrawn in the evening through the water, and replaced, on the following morning, by others recently gathered. These operations were repeated for four days on the same air, to guard against any change occurring in the fruit, and also against leaving it in the air when the sun had withdrawn. On the evening of the fourth day, after forty-eight hours exposure of the vessel to the sun, the bulk of air was somewhat increased by an addition chiefly of oxygen gas and a little azote, without any trace of carbonic acid.

Before the experiment, the air was composed of		*After* the experiment.	
Oxygen, -	207.9	Oxygen, - - -	223.97
Azote, -	782.1	Azote, - - -	789.53
Carbonic Acid, -	0.0	Carbonic Acid, - -	0.0

The addition of oxygen is to be ascribed to the carbonic acid existing in the fruits, and with them transported and decomposed in the vessel.

Thus then the green fruit of peas converts the oxygen of the air into carbonic acid during the night; and, when exposed to the solar rays during the day, it reconverts this acid into oxygen gas.

M. De Saussure also found, that if these bodies were placed alternately in sunshine and in obscurity for two entire days and nights, the air in the recipient then underwent successive changes, which nearly counterbalanced each other; so that, at the close of the experiment, it had suffered no other degree of change than might be attributed to errors of observation.

Still farther to evince the decomposition of carbonic acid by green frui [illegible] next placed a similar quantity of peas in an artificial g [illegible] e, composed of atmospheric air and carbonic aci [illegible] them, with the same precautions, of changing the fruit, to the action of the sun for four days. This gaseous mixture consisted of,

Before the experiment,		*After* the experiment,	
Oxygen Gas,	203.7	Oxygen Gas,	258
Azote,	76.63	Azote,	773
Carbonic Acid,	80.0	Carbonic Acid,	21
	1050.0		1052

So that 59 parts of carbonic acid had disappeared during the experiment, and were replaced by 54.3 of oxygen gas. When a similar experiment was made without renewing the fruit, the oxygen gas was increased in 48 hours from 210 to 239 parts; and the carbonic acid reduced, during the same period, from 50 to less than 8 parts.

Having obtained these results with peas, M. De Saussure next had recourse to a variety of plum (Reine Claude or green-gage), employed by M. Berard. This fruit was gathered when quite green, about five weeks before coming to maturity. When exposed to the solar rays in spring water, these plums produced oxygen gas, and diminished greatly the proportion of carbonic acid, but in a degree less than the pease, from the smaller extent of their surface. The leaves of this plum afforded similar results when placed in the same circumstances. In like manner, when confined in air through the night, this fruit destroyed its oxygen, and produced carbonic acid; and, on the contrary, under exposure to the solar rays, it augmented the proportion of oxygen by decomposing the carbonic acid retained in its substance. Again, by alternate exposure in obscurity and sunshine, the total changes in the air were inconsiderable; and in an artificial atmosphere that contained $\frac{1}{10}$th its bulk of carbonic acid, the whole of that acid, during four days exposure to the sun, entirely disappeared, and was in great part replaced by oxygen gas.

A month after the preceding experiments, others were made with the same variety of fruit, when within two or three days of its maturity. It had now nearly doubled its size, and being in this state confined, for twelve hours through the night, in a recipient of common air, it destroyed a portion of its oxygen, but less in quantity than at an earlier period of its growth. In sunshine also, during twelve hours exposure, it reduced, in a small degree, the quantity of oxygen, and formed carbonic acid. In an artificial mixture of gases, containing $\frac{1}{20}$th part of carbonic acid, in which the experiment was continued through the day and night for four days, with the same plants, the carbonic acid in great part disappeared, and the oxygen was in a small degree diminished.

Experiments were next made on the crab apple (*Pyrus malus*), which, though it be green externally, has within a white colour. When exposed to the sun in vessels of spring water, it almost entirely decomposed the carbonic acid, and produced a corresponding quantity of oxygen gas. In experiments with this and similar fruits a weak sun is required; for a very vivid one excites fermentation, and gives rise to different results. The leaves of the same tree disengaged oxygen very rapidly if the sun was vivid, and decomposed entirely the carbonic acid gas. When confined in a recipient of air through the night, the fruit, as in other instances, reduced the quantity of oxygen, and formed carbonic acid; and when exposed alternately to night and day for forty-eight hours, the air lost only a very small portion of oxygen. In artificial mixtures, which contained about $\frac{1}{20}$th part of carbonic acid, the proportion of oxygen was augmented, and the quantity of carbonic acid greatly reduced. Except only in extent of power, apples, therefore, exert the same action on the air as their leaves.

Green grapes were next exposed to the sun in spring water, and produced an air, which, in 100 parts, contained 29 parts oxygen and 71 azote. The leaves of the grape, in the same circumstances, produced almost precisely the same effect; and similar results were obtained with the green stems of the vine. A bunch of green grapes, confined in air through the night, diminished its oxygen and produced carbonic acid. In the sun also, after forty-eight hours exposure to a strong light, the oxygen of the air was diminished by sour grapes, and a portion of carbonic acid formed; and when alternately exposed by night and day, for four days, to the successive action of light and darkness, the oxygen of the air was partially destroyed, and a little carbonic aci [illegible] If the grapes remained attached to the branch, whi [illegible] he vessel and exposed to the successive action [illegible] ness for twelve days, the air of the vessel then contained no carbonic acid, but a small excess, equal to $\frac{1}{100}$th of oxygen gas; during this time the grapes had tripled their bulk, and exhaled a considerable quantity of water. The leaves also, which adhere to a plant having its root in the soil, afford a like result. In an artificial atmosphere containing $\frac{1}{20}$th of carbonic acid, a detached bunch of grapes, exposed alternately to the sun and darkness for four nights and days, decomposed almost all the acid gas, and augmented the oxygen. During the night, this fruit consumed less oxygen as it approached maturity, and disengaged also less oxygen in sunshine through the day. When confined for four days and nights, and placed alternately in sunshine and shade, it produced little or no change in the air at the period of maturity. To make a comparative estimate of the effects produced on the air by sour and ripe grapes, two experiments were simultaneously instituted and kept up for five nights and days. They gave the following results:

	Atmosphere of the green and ripe grapes *before* experiment.	Atmosphere of green grapes *after* experiment.	Atmosphere of ripe grapes *after* experiment.
Oxygen gas,	210	199.4	202.6
Azote,	790	795.6	792.1
Carbonic acid,	0	0.0	7.3
	1000	995.0	1002.0

These results, obtained three times over, indicate that the portion of oxygen removed by the green grapes is not represented by a corresponding quantity of carbonic acid; while the ripe grapes, according to M. De Saussure, appear not to act on the oxygen, since it is evident that the small portion of carbonic acid left in the recipient might have been decomposed in this, as it was in a former instance.

That grapes nearly ripe still retain the power of decomposing carbonic acid, M. De Saussure farther satisfied himself by the the following experiment. He confined a portion of this fruit when nearly ripe, in an artificial atmosphere, which contained $\frac{1}{20}$th of carbonic acid. The experiment was continued for four days and nights, at the end of which time, nearly two-thirds of the acid gas had disappeared, and a portion of oxygen gas was produced. And from this and other experiments, it is inferred, that green fruits decompose much less carbonic acid than green leaves in the sun, but much more than when they approach maturity.

From the foregoing experiments, the author concludes, generally, that green fruits act upon the air, both in sunshine and in darkness, exactly like green leaves, but in a smaller degree: that, during the night, they cause the oxygen of their atmosphere to disappear, and replace it by carbonic acid, which they in part absorb, but that this power is exerted in a less degree as the fruit approaches maturity: that, on the contrary, when exposed to the sun, they set free, in whole or in part, the oxygen of the carbonic acid absorbed through the night, and leave not a trace of this acid gas in their atmosphere: that green fruits, detached from the plant, and placed alternately in sunshine and in darkness, produce little or no change, either in the purity or volume of their atmosphere: that in sunshine, fruits also possess, like leaves, the power of decomposing carbonic acid, when added artificially to their atmosphere, but that this faculty of decomposing carbonic acid diminishes as they approach maturity.

Beside these conclusions, which seem to be fully warranted by the experiments recited above, M. De Saussure seems to think, that, in their sour state, green fruits assimilate a portion of the oxygen of the carbonic acid which they decompose; while, as they approach maturity, no such assimilation of oxygen takes place. And thus the passage from the acid to the saccharine state appears to shew, that the acidity of the green fruit has relation to the fixation of atmospheric oxygen, and that this acidity disappears when the fruit derives only carbon from the air. The author believes also, that, during their growth, fruits appropriate oxygen and hydrogen, the elements of water, by causing that fluid to lose its liquid state. These opinions must be regarded as resting on more doubtful evidence.

In concluding his essay, M. De Saussure suggests some cautions in conducting the experiments, the neglect of which may have given rise to the erroneous results obtained by M. Berard. The volume [illegible] employed in these experiments on fruits should exc [illegible] orty times that of the fruit; and the *heating* po [illegible] ays should be moderated. If these precautions [illegible] any fruits vitiate the air, even in sunshine, by forming carbonic acid. In the experiments of M. Berard, the fruits were confined in a space that did not exceed but six or eight times their volume, and the vessel was so narrow that they must have suffered from contact with its heated sides.

ART. XII.—*Meteorological Observations made at Port Antonio, on the North Coast of Jamaica,* 1819 *and* 1820. *By* WILLIAM ARNOLD, M. D., Member of the Wernerian Society.*

I DID not at first think of illustrating the annexed tables with more observations than were to be seen at one glance †, as these shew the various peculiarities and phenomena of the north side of Jamaica; nor did I intend publishing them, but having commenced the observations for my own satisfaction, at an early period, I was encouraged to carry them on, with the view of ascertaining two important facts,—First, Whether the moon had any particular influence over the seasons; and Secondly, Which of the seasons were favourable, or unfavourable, to European constitutions.

The prevailing winds, on the north side of Jamaica, are nearly uniform throughout the year; that is, one year resembles another, with regard to seasons, which are always anticipated, and the planters regulate their crops accordingly. From what I have observed during seven years' careful observation, I am enabled to say, with some degree of certainty, that, from the month of January to the month of March, strong northerly winds may be expected, which continue to blow hard for three, five, and seven days, without ceasing day or night, not unfrequently accompanied by squalls of rain and hail.

April, May, and June, are generally mild; and the sea-breeze from the east, varying a point or two to the northward and southward, uniformly sets in about 9 A. M.; and sometimes a moderate refreshing current until 5 P. M. This is generally succeeded by cool, exhilarating breezes from the land, almost all the night;—thus tempering the day and night, so as to be endured by the European without inconvenience or danger, if he does not incautiously expose himself at night.

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* Read before the Wernerian Natural History Society, 15th Dec. 1821.

† The Tables, as drawn up by Dr Arnold, contained various particulars respecting the weather, which could only have been given in an engraving of a very inconvenient size.—ED.

July, August, and September, are the most sultry months throughout the year, and are always calm, both day and night. The breezes that generally favour us during the day, set in so late, and are so light, that the inhabitants are glad to keep within. There is little or no breeze at night; and, as may be seen from the Tables, the thermometer is higher at this than at any other period during the year, being not unfrequently known to rise, when exposed to the sun, to the incredible height of 135° of Fahrenheit. There is very little variation during the twenty-four hours; the mercury seldom falls in the night more than 4° or 5°. Lightning and thunder are very frequent and terrific during these months, and often do considerable mischief, if unaccompanied by rain. This, however, is seldom the case on the north side of Jamaica.

I look upon this period to be the most trying to the European constitution. The days are sultry; and the nights are equally so,—preserving almost one uniform and continued oppressive heat for the twenty-four hours.

If any season calls forth the energy, the prudence, and cautious care of the individual, it is this; for it is but too obvious, that what might be done with impunity in the cooler months of January, &c., would occasion certain death in this period.

October, November, and December, are the months when rain may be expected, which never fails to cool the air, and is certainly of astonishing benefit. This is designated *the rainy season*. It sets in about the middle or latter end of October, and continues until the approach of northerly winds, which are sometimes known to prevail in December, and are by no means unwelcome.

These are the months that are also looked upon as the *hurricane period*. Earthquakes, tempests, and storms, frequently take place. Some awful and desolating visitations have occurred in different parts of the island, leaving melancholy records of innumerable m

The perio nth of November to April I look upon as exce The prevailing winds, and the comparatively e, render the climate nearly similar to that of the south of France, and consequently very tolerable to every constitution.

In the year 1815, a very remarkable circumstance was observed respecting the height of the thermometer. On the 18th October a dreadful hurricane visited all parts of the island, accompanied by two severe shocks of an earthquake. During the continuance of this alarming visitation the mercury scarcely varied a degree. At noon, on Wednesday, it was seen at 75°, and not until the afternoon of Friday did it alter, when it rose to 76°, whilst it may literally be said to have blown from every point in the compass, with increasing torrents of rain.

TABLE shewing the State of the Thermometer at Port Antonio, taken twice a-day, at Sun-rise and Noon, for the year 1819.

	JAN.		FEB.		MAR.		APR.		MAY.		JUNE.		JULY.		AUG.		SEPT.		OCT.		NOV.		DEC.	
Days.	Sun-rise.	Noon.	Sun-rise.	Noon.	Sun-rise.	Noon.	Sun-rise.	Noon.	Sun-rise.	Noon.	Sun-rise.	Noon.	Sun-rise.	Noon.	Sun-rise.	Noon.	Sun-rise.	Noon.	Sun-rise.	Noon.	Sun-rise.	Noon.	Sun-rise.	Noon.
1	76°	80°	70°	80°	70°	80°	70°	80°	70°	82°	74°	84°	82°	86°	74°	84°	76°	85°	76°	85°	76°	82°	76°	84°
2	76	80	70	80	70	80	70	80	70	82	74	84	76	86	74	86	74	84	74	86	76	84	74	82
3	76	80	70	80	70	80	70	80	70	80	74	84	77	86	75	85	76	85	75	84	76	84	74	82
4	74	80	68	79	68	78	70	80	70	80	74	84	75	85	75	84	77	85	76	84	76	85	74	82
5	74	80	68	78	68	78	70	80	72	82	74	84	74	87	74	84	76	85	76	85	74	85	74	82
6	74	80	66	78	68	80	70	80	72	82	74	84	78	86	74	84	76	86	74	85	72	84	74	80
7	74	80	68	78	68	80	70	80	74	82	74	84	73	86	74	84	76	84	74	86	72	84	72	80
8	74	80	68	78	68	80	70	80	74	82	74	84	77	85	74	84	76	84	74	86	72	84	70	80
9	76	80	70	80	68	78	70	80	73	84	74	84	76	85	74	84	76	84	74	84	76	84	70	82
10	76	80	70	80	68	80	70	80	73	84	74	84	78	84	74	84	76	85	74	84	76	84	70	82
11	76	80	68	78	68	80	70	82	72	82	74	84	78	84	74	84	76	86	74	84	76	84	70	82
12	76	80	68	78	68	80	70	82	73	84	74	82	77	85	74	84	77	85	74	85	76	85	70	82
13	76	80	68	78	68	80	70	80	73	84	74	82	76	85	76	84	76	85	76	84	76	84	68	80
14	75	80	70	78	68	82	70	80	72	82	74	84	80	86	76	84	78	86	74	84	76	82	70	80
15	75	80	70	80	68	80	70	80	72	83	70	82	77	83	76	85	78	86	74	86	76	84	70	82
16	74	80	70	80	68	82	70	81	74	84	74	84	75	85	76	85	76	86	76	86	76	84	70	82
17	76	80	70	80	68	82	71	81	74	84	74	84	74	82	76	80	75	84	76	86	74	85	70	82
18	74	80	70	80	68	80	70	80	74	84	70	85	74	82	76	82	74	86	74	84	74	84	68	80
19	73	80	70	80	68	80	70	80	74	84	77	84	74	82	76	82	76	86	74	84	74	84	68	80
20	76	80	70	80	68	80	70	80	74	84	75	84	74	80	76	84	78	86	76	84	74	84	68	80
21	74	81	70	78	68	80	70	80	74	84	78	84	74	84	78	84	77	86	76	84	74	84	68	80
22	73	81	70	78	68	82	70	80	74	84	75	84	74	84	78	86	74	84	74	84	75	84	72	80
23	73	81	70	80	68	82	70	80	74	84	71	82	74	86	78	84	74	86	76	85	76	84	74	80
24	73	81	70	78	68	82	70	80	74	82	73	85	74	84	76	84	76	84	76	85	78	84	74	80
25	73	81	70	78	68	80	70	80	74	82	73	85	74	85	78	84	76	86	74	84	78	84	70	80
26	73	81	70	80	70	82	71	82	74	82	73	84	76	85	76	84	76	87	76	85	76	84	70	80
27	73	80	70	80	70	80	71	82	74	80	73	84	74	84	76	84	74	85	76	84	76	84	70	80
28	72	80	70	80	70	80	72	82	74	82	73	80	74	84	.6	84	74	86	76	84	76	84	70	80
29	72	80			70	80	71	82	74	82	73	83	74	84	76	84	76	86	76	84	76	84	70	80
30	71	80			70	82	71	82	74	82	78	83	74	84	76	84	76	86	76	85	76	84	70	80
31	71	80			70	80			74	84			74	84	78	84			76	84			72	80

TABLE shewing the State of the Thermometer at Port Antonio, taken twice a-day, at Sun-rise and Noon, for the year 1820.

	JAN.		FEB.		MAR.		APR.		MAY.		JUNE.		JULY.		AUG.		SEPT.		OCT.		NOV.		DEC.	
Days.	Sun-rise.	Noon.	Sun-rise.	Noon.	Sun-rise.	Noon.	Sun-rise.	Noon.	Sun-rise.	Noon.	Sun-rise.	Noon.	Sun-rise.	Noon.	Sun-rise.	Noon	Sun-rise.	Noon.	Sun-rise.	Noon.	Sun-rise	Noon.	Sun-rise.	Noon.
1	70°	80°	68°	80°	70°	80°	74°	80°	72°	80°	78°	82°	76°	84°	76°	84°	76°	84°	78°	85°	76°	84°	72°	78°
2	70	80	68	80	70	80	68	80	72	82	78	82	76	85	76	84	76	84	78	85	76	84	74	80
3	70	80	68	80	70	80	69	80	74	82	76	82	76	84	74	84	76	84	78	85	76	84	72	80
4	68	78	68	80	70	80	72	80	72	82	78	82	76	84	78	79	76	84	76	85	76	84	70	78
5	68	78	70	80	70	80	73	80	74	82	78	84	76	84	72	82	76	84	78	84	74	80	70	80
6	68	78	70	80	70	82	74	80	72	82	78	84	76	80	75	84	76	82	78	84	73	82	68	78
7	68	80	70	80	70	80	74	80	70	80	74	82	76	84	76	80	76	79	78	82	73	84	68	78
8	68	80	70	80	70	80	74	79	70	80	78	84	76	82	74	84	74	84	78	84	74	84	68	80
9	68	80	70	80	70	80	74	79	70	80	78	84	76	84	74	84	76	86	78	84	72	84	70	80
10	68	80	70	80	72	80	74	76	70	80	79	85	76	84	76	84	76	86	78	84	74	84	68	80
11	68	80	70	80	72	81	74	76	70	80	79	85	76	84	75	84	76	86	76	84	74	82	68	78
12	68	80	68	80	72	82	72	74	72	82	79	85	78	84	75	81	76	86	72	82	74	82	68	78
13	68	80	70	80	72	82	72	78	72	82	76	84	78	84	75	84	76	86	76	82	74	84	70	76
14	68	80	70	80	72	80	72	78	72	82	74	84	76	84	74	84	76	84	76	80	74	84	72	80
15	70	80	70	80	70	80	70	79	72	82	76	84	74	82	74	84	76	84	78	80	74	84	72	80
16	70	80	70	80	70	80	72	80	72	82	76	84	76	84	74	84	76	85	78	85	72	82	72	81
17	69	80	70	80	70	80	72	78	72	82	76	84	76	82	74	84	76	84	76	84	74	84	72	80
18	69	80	70	80	70	80	72	78	73	82	76	84	76	82	74	84	76	86	76	80	72	84	72	80
19	68	78	70	80	70	80	72	76	73	80	76	84	78	82	74	84	76	86	78	84	72	84	72	80
20	68	78	70	80	70	80	70	74	73	80	76	84	76	84	74	84	76	85	78	85	74	80	72	80
21	68	78	68	80	70	80	70	71	73	76	76	84	76	82	74	84	74	85	78	85	74	80	72	80
22	68	78	68	80	70	80	70	72	73	80	76	84	76	82	74	84	76	85	78	84	74	80	70	78
23	68	80	70	80	70	80	72	74	74	78	76	84	76	82	74	84	76	85	78	80	72	80	70	80
24	68	80	70	80	70	80	72	80	74	80	76	84	72	80	74	84	74	85	78	80	72	81	72	80
25	68	80	70	80	71	80	73	80	74	80	76	84	74	84	74	84	76	85	78	82	72	81	70	80
26	68	80	72	80	69	80	70	79	74	84	76	84	74	82	74	84	76	84	78	80	72	80	68	80
27	68	80	72	80	68	79	72	80	74	84	76	84	76	84	74	84	76	85	78	80	72	79	68	78
28	68	80	72	80	68	80	70	80	74	82	76	84	74	84	73	84	76	85	78	84	74	78	70	80
29	70	80	72	80	70	80	70	78	74	82	76	84	76	84	74	84	76	86	80	82	74	78	72	80
30	70	80			70	80	70	80	72	82	76	84	76	84	74	84	78	86	78	80	74	78	72	78
31	70	80			70	80			74	82			76	84	74	84			78	84				

Observations on the preceding Tables, by Dr BREWSTER.

If we assume that the temperature at Sun-rise and Noon gives an approximate value of the mean temperature of the day, we shall have the Mean Temperature of the different months, at Port Antonio, as follows:—

Mean Temperature of Months for 1819.

January, - - -	77.2	July, - - -	80.0
February, - -	74.2	August, - - -	79.8
March, -	74.4	September, - -	80.8
April, -	[illegible]5.3	October, - - -	79.6
May, -	[illegible]	November, - -	79.6
June, -	[illegible]	December, - -	76.0

[illegible]ture of the year 1819, - 77.78

burg and of Oedenburg; but it is more particularly between Debretzin and Nagy-varad that it has long been obtained, in lakes which dry up in summer, and where it also effloresces at that season on the surface of the ground. These saline efflorescences, which in the middle of summer resemble heaps of snow, have given rise to the name of White Lakes (Fejer to) which is applied to those marshes. They are renewed at the end of three or four days after being removed; so that, during the whole summer season, considerable quantities are collected, which are afterwards carried to Debretzin, as well for the manufacture of soap as for exportation. It appears that the quantity collected annually amounts to upwards of 10,000 quintals, and an infinitely greater quantity might be obtained, as there are very many rich lakes which are neglected on account of the remoteness of their situation.

Similar Lakes known in other Parts of the Globe.—The existence of natron in the midst of plains, in the waters of lakes and marshes which cover them, is one of the most interesting facts in geology. It is not in Hungary only that this phenomenon presents itself; we find it every where in the midst of the vast deserts which occur in so many places on the surface of our globe. All that we know of this mineral production in Egypt, in Arabia, in Persia, in India, in Thibet, in China, in Siberia, in the plains of the Caspian and Black seas, in Asia Minor, and at Mexico, evinces that every where it occurs with the same relations, and in the same circumstances; every where is it found in the midst of sands mixed with marl and clay, and is accompanied with many other salts, of which the most constant is common salt. Those natron lakes of Hungary which are the nearest to us, and those which may be visited with most facility, were well adapted to excite my curiosity; but it is very difficult to collect, in the midst of those plains, which do not present a single ravine, a sufficient number of facts to lead to the solution of all the questions which might be proposed. I have returned to Debretzin with as little certainty on this subject as when I left it, even after perusing the indications presented by various publications regarding it; the continual rains which had previously taken place, had so broken up the ground, that it was with much difficulty I could proceed to verify the principal circumstances which have been described.

Situation and description of these Lakes.—It is between Debretzin and Nagy-varad, and especially on the heaths in the neighbourhood of Kis Maria, that the lakes from which the natron is obtained may be best seen. The whole ground about them is covered with *salicornia*, *salsola*, and many other maritime plants, which are also collected for the purpose of extracting the salt by burning. The soil in which these plants grow is a micaceous quartzy sand, of a whitish or greyish colour, impregnated with saline matters. At the margin of the lakes there is found a clayey substance, of a grey colour, becoming black when moistened, which is always more or less mixed with gravel. It appears that it is this matter which forms the soil of the parts where the waters are more particularly collected. It effervesces with acids, even after the carbonate of soda has been removed by lixiviation, which is caused by the presence of a little carbonate of lime, the quantity of which was six parts in the hundred in the specimens which I collected. These lakes or marshes, which in general are very shallow, dry up almost entirely in summer; but when I visited them, they were filled with water, in consequence of the rains which had taken place on the preceding days. These waters were turbid, of a greyish colour, and presented a slight tinge of red, when, on settling for some time, they had deposited their mud.

These are the only observations which I have been able to make, although I walked along the margins of these lakes during a whole day; but the surface is every where continuous, and not a single ravine occurs to enable one to study with more precision the composition of the soil. It will be necessary, in order to understand the phenomena presented by this soil, to dig some pits, or to take advantage of those which have occasionally been made for wells in the neighbourhood. Ruckert, who was for a long time employed in extracting the natron, and who had opportunities of more particularly examining the soil, asserts that the sand sometimes includes pisiform iron-ore, that it is never more than four or five feet deep, and that it rests on a bed of blue clay. He informs us, also, that the greater number of the lakes are dried up at midsummer, and that the natron which effloresces at the surface is then collected. The efflorescence is renewed at the end of three or four days, and it may then be collected anew; so that a great quantity may be collected in the course of a summer. But there are deeper parts where the water never dries up, but contains a great quantity of carbonate of soda (50 or 60 in the 100, according to Ruckert), which crystallises during the cold nights of autumn. This saturated water is conducted to the manufactories, and reserved for the labour of winter.

Examination of the Natron collected.—Not having an opportunity of seeing the efflorescences on the ground where they are formed, I examined the natron which had been previously collected. It was mixed with a pretty considerable quantity of grey argillaceous matter, and contained much muriate of soda, as well as a certain quantity of sulphate. I had afterwards an opportunity of seeing it with the peasants in the *Grande Cumanie*, where it was also gathered in the marshes which border the Theiss, and it contained the same salts also, although in less quantity. I made the same observation on the natron gathered in the plains of the Lake of *Neusiedel*. It therefore appears evident, that the carbonate of soda is never pure, and that in Hungary, as in all the other places where it is found, it is always mixed with muriate of soda, in greater or less quantity.

Origin of the Natron.—Such are the facts which I have been able to collect in the midst of those desert plains, where the geologist, being able to examine only the uppermost pellicle of the globe, finds himself suddenly arrested in the progress of inquiries which might prove of the greatest interest. The primary object which presented itself here, was to determine the origin of this immense quantity of natron which is daily efflorescing at the surface of the earth, and which is found every where in the waters which cover the plains of Hungary. But the data which we possess cannot permit us to say any thing with certainty on this subject, and we are limited to more or less probable conjectures, which, however, deserve a certain degree of attention, [illegible] are deduced from facts, and rest upon no hypoth[illegible] was of opinion that the sub-carbonate of soda oc[illegible]rmed in the sand or clay, at a certain depth, along with the different salts with which it is mixed, and that the waters, by filtering through the mass of earth, in order to regain their level, laid hold of those substances, which they carried to the surface of the soil. This opinion is, in fact, that which would naturally present itself to the first observers, because it is the most simple; but it cannot be supported by any positive observation, because no pits have been dug which might confirm it, in the different places where the natron appears at the surface of the soil. But farther, there is no analogy to give it countenance; for nowhere, in the deposits of rock-salt, whether of the more ancient or more modern kind, is carbonate of soda found; and, lastly, the waters of our seas which deposit their salt upon the shores, are equally destitute of any traces of it. Now, we find, however, that in this latter case, natron also is formed, although in small quantity, efflorescing at the surface of the soil; and here its origin cannot be attributed to any thing else than the decomposition of the muriate of soda. We know in fact that this decomposition may take place through the agency of several means which differ from each other only in the quickness or facility with which they operate. Advantage has been taken of this in the manufacture of artificial sub-carbonate of soda, in which several methods, more or less advantageous, have been successfully employed.

It is, therefore, also in the natural decomposition of muriate of soda, that we are led to seek the origin of the natron which occurs in such abundance in the vast plains of our Continents. It is thus that M. Berthollet has explained in a manner extremely probable, the formation of this salt in the valley of the natron lakes in Egypt. This learned chemist has shown, that it is owing to the reciprocal action of the muriate of soda and carbonate of lime, assisted by the efflorescence, which determines the successive separation of the carbonate of soda; and which, by this means, allows the decompositon to go on *ad infinitum*. The inspection of the places gives every probability to this explanation; for the lakes contain a great quantity of muriate of soda, and they occur in the midst of a calcareous formation, the rocks of which project here and there through the sand which cover them; masses or beds of gypsum also occur, which probably accompany the deposits of rock-salt which the waters traverse before arriving at the lakes. This explanation appears to me also to agree perfectly with the nature of the lakes of Hungary; for it is to be remarked, that the richest of them occur in the eastern part of the great plain, at a short distance from the foot of the calcareous mountains, which form the advanced posts of the great mountains of Transylvania; and in the middle, or at the back part of which, there are found considerable masses of salt. Farther to the west, the plain is filled with deposits of coarse marine limestone, similar to those of the neighbourhood of Paris, which are seen in many parts about Pest, and which, probably, extend deep beneath the sand, which covers them from observation. The carbonate of lime appears to be, besides, very abundant through the whole plain, since it is daily deposited at the bottom of the marshes, where it forms beds of greater or less solidity. Lastly, In the plains of Sthulweissenburgh and Oldenburg, where natron equally occurs, there still exists considerable mountains of compact limestone, or deposits of coarse marine limestone with shells.

Thus, in order to explain the daily formation of natron, it is only necessary to admit, as every thing indicates, that a great quantity of muriate of soda exists in all the plains of Hungary. But it is, perhaps, possible to go farther, and to trace these saliferous deposits to those which exist in so great abundance in Transylvania and on the frontiers of Marmaros. It is evident, in the first place, that the salt deposits of *Sziget* and of *Rhonazek*, are prolonged into the valley of Theiss; that they are those which occur at *Talaborfalva*, at *Sandorfalva*, and which produce the salt springs of *Huszt*, *Visk*, &c. These same deposits occur in the valley of *Szamos*, as well in the mountains which form the right side, as in those which are seen on the left, and which stretch to the plains of Hungary. After this, it becomes almost impossible not to assent to the idea, that the marshes of the county of *Szathmar*, which are found in the same line, and at a short distance from the last works, should also have the property of furnishi [illegible] muriate and carbonate of soda to the same saliferous d [illegible] would form in this place the soil of the plain, whe [illegible] be necessarily covered with sandy deposits. T [illegible] ily applies to the counties of *Szabolts* and *Bihar*, which are contiguous with the former: and it is impossible that the argillaceous matters which occur at the margin of the lakes near Debretzin, could be any thing else than saliferous clay. I may remark with regard to this, that the greater number of saliferous clays which I have had an opportunity of seeing, contain, like the argillaceous masses of the margins of the natron lakes, a certain quantity of carbonate of lime, and that they are more or less sandy. These mixtures naturally assist the decomposition of the muriate of soda, the one, by directly furnishing the substance which might produce it, and the other, by rendering the mass more porous, and thus facilitating the efflorescence of the natron. If decomposition does not take place in mines, it is, without doubt, because the heat is not sufficiently great, nor the moisture copious enough; and especially because the air cannot have a sufficient circulation. It is probable, that on exposing to the day the refuse of salt mines, they would also form carbonate of soda. However this may be, it is not the less remarkable, that on following these plains, where the muriate of soda is constantly found in the waters, we arrive, without discontinuity, at masses of salt, which are the object of considerable works.

Sulphate of Soda.—Admitting in this manner the prolongation of the saliferous deposits in the plain, the formation of sulphate of soda may be easily explained; for it is known that the saliferous clays always contain nests of gypsum or sulphate of lime of greater or less size. Now, it appears that the muriate of soda must be decomposed by the sulphate of lime, in the same manner as by the carbonate; the result must be sulphate of soda, which effloresces also at the surface of the ground. We know, from the experiments of *Gren*, that this decomposition is particularly active in winter, so that it must take place particularly in the waters (*eaux mères*) which remain at the bottom of some lakes. It is probable that it was to a decomposition of this kind that the sulphate of soda was owing, which formed at Dieuze, in the *schlot* and residue of the coppers, thrown out in heaps of considerable size; at the foot of which, M. Gillet de Laumont observed, in the winter of 1790, a spring of water impregnated with sulphate of soda. But besides the sulphate of lime, it is possible that there exists in the waters a considerable quantity of sulphate of magnesia, the decomposition of which by the muriate, equally at a low temperature, is still more easy. Gren discovered that the great quantity of sulphate of soda, which formed in the salt basins of Saxony, during the winter of 1794, was chiefly owing to sulphate of magnesia. It is to be remarked, that there also in fact exists much sulphate of magnesia in the plains of Hungary, to which little attention is paid.

Saltpetre.—There exists, in the plains of Hungary, another salt, whose origin is perhaps of more difficult explanation: this is saltpetre, which occurs, as it would appear, in very great quantities; it effloresces also at the surface of the ground in the counties of Szathmar, Szabolez, and Bihar, as well as in the plains of Stulhweissenburg and Oldenburg: a sufficient quantity might be collected to supply all Hungary and Austria.

In 1802, nearly 7000 quintals were extracted for government, although workhouses were not erected for collecting all that might be got.

Ruckert thought that this salt must be a mineral production, and he was led to believe that there existed a bed of more than sixty leagues in length by twenty-five or thirty in breadth. The principal facts on which he founded this opinion were, that all the wells which have been dug in the eastern part of the great plain, afford only nitrous waters, and that the saltpetre which effloresces at the surface of the ground, occurs in places where it does not appear that animal substances could have contributed to its formation. I must still dissent from him with regard to this; for I observe, on the contrary, that there are in this country immense herds of oxen, of buffaloes and horses, which must contribute in a great degree to the production of this salt. It appears to me still, and even after what Ruckert says, that the waters of the subterranean springs contain but a very small quantity of this salt; and it would not be surprising were it found to be the result of the infiltration of the surface-water. As to the rest, the existence of saltpetre in the midst of heaths, and in a [illegible] of other circumstances, is a phenomenon which stil [illegible] tigation. We know that this salt forms in abunda [illegible] where animal and vegetable matters in a state [illegible] are found united; but it is not certain that what is naturally formed is always in the same case. It is especially very difficult to adapt this explanation to the formation of nitrate of potash in certain caverns of calcareous mountains, as, for example, in the nitre-mine of *Molfetta* in Poland, and in others which occur in many parts of the same country.

On the Pitchstone of Saxony.

Valley of Tribisch.—I come now to the Valley of *Tribisch*, which opens into that of the Elbe, at Meissen, and which is deserving of particular attention, on account of the great masses of pitchstone which it presents to the observer. On entering the valley by Meissen, the first rock that occurs is syenite passing into granite, like that of Dohna; but at the distance of three quarters of an hour's walk, a particular kind of porphyry begins to appear, of a brown and sombre hue, which includes a great number of small crystals of lamellar felspar, sometimes even a little cracked, and resembling glassy felspar; brown mica with much lustre, also occurs in small hexagonal lamellæ. These rocks attract the magnet, and have a certain analogy with some varieties of porphyritic trachyte; in some parts, they have a tendency to divide into thin folia; in others, the mass is less divisible, and assumes an earthy character. These porphyries constitute a great part of the mountains which border this valley, and continue a great way on ascending toward Freyberg, until at last we arrive at arenaceous deposits of argillaceous matters, having more or less of a porphyritic structure, (Thonstein, porphyrartiger Thonstein).

But, in following this valley, we find on its left a small transverse valley, where the pitchstones more particularly occur. They form a pretty large mass of rocks, raised upon a considerable elevation, and which, from its position, appears to rest upon the porphyries just mentioned. In the bottom of the valley, near the baths which have been erected there, we find an argillaceous rock, with a white or reddish basis, which presents spots of a deeper red colour and irregular figure, and in which crystals of felspar and quartz are observed here and there. These are claystones, (Thonstein), more or less distinctly porphyritic (Mimporphr, Brong.), perfectly similar to those which occur beyond Freyberg, in all the hills around Kremnitz, and which belong to the red sandstone formation (formation des grès rouges). Above the baths, we begin to find the pitchstones, which present an infinite number of varieties. We observe all the transitions from rocks perfectly vitreous, and with a resinous lustre, to rocks entirely earthy (pierreuses), the greater number felspar, the others siliceous, presenting every tint of colour, brownish-red, reddish-white, greenish, &c. All of them, and especially the vitreous varieties, include crystals of lamellar felspar, more or less numerous, which are even occasionally a little split, and resemble glassy felspar. We find also, but very rarely, hexagonal lamellæ of brown mica, having a great degree of lustre. In general, these rocks have the greatest analogy with certain varieties of obsidian (perlite), which we have observed in the Valley of Glasshütte. The only difference that I could observe, is, that here there was not the least indication of porous substances or pumice, nor are the nodules or globules radiated from the centre to the circumference, which are so common in the obsidian, to be seen either in the vitreous masses, or in the varieties which become earthy.

But, if the Valley of Tribisch is extremely interesting as a place for studying all the varieties of which pitchstone is susceptible, it is not equally so with regard to the relative position of this rock. It is impossible, with reference to this, to determine any thing with precision. It might in general be supposed, from the mutual position of the masses, and the mineralogical relations which their different varieties present, that the claystone porphyries, and the pitchstones, belong to the same formation, and that they both rest upon the dark-brown porphyries which constitute the principal mass of the country. Fortunately, the relations are more distinct in other places, which are not so far removed from this as to give rise to the supposition that the rocks belong to a different formation. The most remarkable locality is near Mohorn and Herzogswald. There pitchstones occur, which do not present a great number of varieties, but which evi[illegible] [illegible]long to an arenaceous deposit, the basis of which is [illegible] (argilolite), of a reddish colour, and very solid [illegible] [illegible]he fragments are of gneiss, mica-slate, and clay-s[illegible] [illegible] commonly angular, small, and more or less numerous. What is very remarkable is, that these fragments occur not only in the argillaceous mass, but also in the pitchstone itself; thus proving, by a mineralogical character that the two rocks, however different in external appearance, belong to the same formation.

Considerable masses of claystone also occur to the south-west of Freyberg, about the small town of Chemnitz. Here the vitreous retinite, in so far as I know, is not found; but, in the midst of the porphyritic claystone, we observe porphyritic rocks, having a basis of compact felspar, with a somewhat resinous lustre, of a brown or reddish colour, sometimes mixed with a little green, which include very small crystals of lamellar felspar, and which have a very striking resemblance to certain earthy varieties of the porphyritic pitchstones of Tribisch. The same porphyries still occur at Planitz, near Zwickau, which is not far distant from Chemnitz, and in the midst of the same formation (terrain), which, in this part, occupies a considerable extent. This porphyry forms kidneys in the middle even of the pitchstone, which has been found here, either in the sandstone of the coal formation (grès houiller), or between it and the red sandstone lying immediately above it, and which is itself covered, in this part, by amygdaloids.

The result of these data is, that the pitchstone is found in immediate connection with the claystone deposits; but these deposits are themselves a part of the red sandstone formation (Roth liegende), which in this country covers a considerable extent, and they would appear to occur sometimes above and sometimes beneath the quartzose arenaceous deposits, of which it is formed. Thus the pitchstones of Saxony belong to the red sandstone formation, and would appear to occupy the place of the porphyries with a basis of compact felspar, which occur in many localities, and which even appear in an evident manner at Chemnitz, where they are solitary, and at Planitz, where they form kidneys even in the midst of the pitchstones. But it is known farther, that it is universally in the red sandstone that the pitchstone occurs. It is in this position that these rocks appear in Scotland, according to the descriptions given by Professor Jameson, Dr MacCulloch, and Dr Boué; it is still in the same manner that they occur at Grantola, on the Lago Maggiore, where I visited them in 1817. There they assuredly form a part of a sandstone deposit which fills the small valley in which Grantola is situated, and which lies to the west upon transition limestones and rocks of mica-slate, stretching toward the east over a considerable extent. These sandstones have all the characters of the red sandstone, and it is remarkable that they present indications of coal, which would seem to give additional strength to this opinion. The following are the varieties which I found at Grantola, in the deposit alluded to :—

1. Quartzose arenaceous deposits, including rolled pebbles of mica-slate and quartz, and abundance of scales of mica. They rest immediately upon the gneiss.

2. Above these sandstone deposits, there occur fine sands more or less argillaceous, in the midst of which there exist alternating beds of more or less compact red matters, which have all the characters of claystone, and which pass insensibly into the sandstones, which have all the characters of varieties of red sandstone. These last sandstones appear to form considerable masses; they occur on both sides of the valley. On the right, above Grantola, they occur associated with conglomerates, having very numerous and very large pebbles, which are yet perfectly similar to those of the red sandstone formation.

3. The species of claystone which I have just mentioned, terminate by forming of themselves hills of considerable size, where they assume still more distinctly the characters of those which we have mentioned at Freyberg. They present in some points a porphyritic structure; in others, they exhibit white spots, which are formed of a very soft substance, like lithomarge. This substance becomes more or less abundant; but it happens that it is destroyed at the surface, being washed away by the waters, and leaving irregular cavities, which give a scoriaceous aspect to the rock; and, in fact, it appears that these varieties have occasionally been considered as true scoriæ.

4. Through successive passages, we come from the porphyritic claystone to a true porphyry of a brown colour, which has the greatest ar [illegible] with those which occur in the similar beds of Chemnitz [illegible] he basis of these porphyries becomes gradually mo [illegible] fracture more conchoidal, and its lustre resinou [illegible], by degrees, to the vitreous rock,

or pitchstone, analogous to those of Tribisch, and of many other places.

But, after having spoken of the general position of pitchstone, it may not be useless to say a few words on the origin which may be attributed to it, following the probabilities which result from our actual recognitions. Some mineralogists consider these rocks as of igneous, others as of aqueous origin. This is always the great question which divides the Schools, and on which it is impossible to agree at all times, when the facts which might lead to the one or the other opinion are not rigorously contrasted. We shall endeavour, in this place, to exhibit a short account of both, that we may be enabled to judge of their respective validity.

In adopting the opinion of an Igneous origin, the principal argument is founded upon the resemblance which exists between the pitchstones and certain varieties of claystone with which they are associated, and products evidently or very probably of igneous origin. It is thus that a very remarkable analogy exists between the vitreous pitchstones and certain varieties of the obsidian of active volcanoes, and a perfect resemblance between these rocks and certain varieties of perlite (perlite retinite), of trachytic formation, which are in all probability of igneous origin. In the same way, on comparing certain earthy pitchstones, both simple and porphyritic, as well as the porphyritic claystones, with certain varieties of earthy perlite (perlite lithoïde), of trachytic porphyry, and of porphyre molaire, we find that there is a mineralogical identity. Now, as the perlites, trachytic porphyries, and porphyres molaires, are very probably of igneous origin, the same probability holds equally with regard to the rocks which resemble them. Such, I believe, are the sole data that can be given with regard to the igneous origin of the masses of pitchstone.

The Neptunian origin is founded upon several data, which we proceed in like manner to state. It is necessary to admit the analogies which we have mentioned as existing between the pitchstones and the rocks of igneous origin, because they are real, and because to reject them would be to refuse assent to evidence; but, it cannot also be admitted, that these are the only analogies which lead to this opinion, and that all the rest are of a contrary nature. Thus, it is a fact, that nowhere do we find in the masses of pitchstones, either porous substances or slags, which exist every where in the currents of obsidian poured out by modern volcanoes, and in the masses of perlite which belong to the trachytic formation. Again, while the earthy varieties of pitchstone, as well as the felspar porphyries with which they occur, have a pretty marked analogy with the earthy perlite, trachytic porphyries, and porphyres molaires, it must be remarked, that an equally perfect identity is not found when we examine at the same time, on all sides, the series of varieties through which all these rocks pass. In fact, we do not see in the pitchstone the immense quantity of small vitreous globules, radiated from the centre to the circumference, which is every where found in the perlites, trachytic porphyries, and porphyres molaires, and of which the quantity is so great, that it might in many cases be said that they are rather globular rocks than vitreous and porphyritic ones. Nor do we find these varieties of pitchstones successively modified till they assume a cellular structure, with hollow cellules, as we find in the rocks with which they are compared.

Thus the comparison of the product, which holds good when some isolated specimens are taken, will not do so when all the varieties are collected for more scrupulous examination. We find, then, that the varieties, which are of the greatest importance in leading to the opinion of an aqueous origin, are precisely those which are wanting. It appears to me, therefore, that, on weighing the preceding statements, in order to discover their comparative validity, we shall find a little more weight in those which have been brought forward in favour of the neptunian origin, since they are founded precisely upon the absence of products, which would give the greatest probability to the hypothesis of an igneous origin. But I shall not even admit this probability, and I shall consider the data which might be premised in favour of either opinion, as mutually compensated, so that, on limiting ourselves to them alone, the geological relations of the rocks in [illegible] must remain undecided.

But, after h[illegible]d the preceding statements, there remain other d[illegible]ot liable to be counterbalanced. These are in fa[illegible]unian origin; they are founded upon relations entirely geological, upon the circumstance that the pitchstones evidently form a part of the red sandstone formation, and are also found in connection with arenaceous deposits. These are conglomerates, having an argillaceous basis, and whose rolled pebbles are of gneiss; fine-grained quartzose sandstones, carbonaceous sandstones, and schistose clays. Now, these substances have evidently been deposited from water, and as the pitchstones occur imbedded in them in nodules or small beds, and since they contain the same fragments of gneiss, as is seen at Mohorn, it becomes impossible to admit, with regard to them, any other origin than that which must necessarily be attributed to the arenaceous rocks.

Thus, without anticipating the probable conclusion which may result one day from new facts furnished by observation, it is plain, that in the present state of our knowledge, the probability is in favour of the neptunian hypothesis. It is, therefore, this which I shall adopt for the present, always pointing out these formations to geologists, who may have an opportunity of studying them, as meriting the most scrupulous attention.

The Mineralogical School of Freyberg.

Freyberg.—We could no longer expect a series of good weather, at the season when we returned, and when Saxony is commonly covered with snow; bad weather also came on, and I could not put in execution other intended excursions, which I had meditated with regard to certain points of geology, which still interested me about Freyberg; even the last excursions which I made presented many difficulties. I could, therefore, no longer interrogate Nature herself; but I found at the school of Freyberg, with the professors and officers of the mines, collections made with great care, and which presented a number of very interesting facts. The genius of Werner still hovers around, and mineralogists and geologists, on entering upon the classical ground, still feel in themselves an increase of that zeal, that love of science, that philosophical spirit, which for thirty years the illustrious professor diffused over all Europe.

The collections of the Academy of Mines presented to me a multitude of very interesting facts, in a geological point of view, and which furnished me with a great number of comparisons between the formations which I observed in Hungary, and those which resemble them in different parts of our globe; the collections of the islands of Paros, Milo, Nipoligo, Patmos, Santorin, &c. have been for me a source of precious comparisons. The collections of different parts of Germany were still well adapted to arrest my attention, and those which M. Freisleben has gathered in Mansfeld and Thuringia, and which form the foundation of the excellent work which he has published on these countries, still presented a mass of details and of geological relations of the highest interest. Lastly, the geological maps of Saxony, which all the lovers of science are anxious to see published, and which M. Kuhn, professor of geology, had the kindness to submit to my inspection, accompanying them with a multitude of details, which are still nowhere to be found, were to me a subject of very instructive investigation.

Art. XIV.—*On Machinery for Calculating and Printing Mathematical Tables**. By Charles Babbage, Esq. F. R. S. Lond. and Edin. &c.

AMONG the brilliant inventions which have distinguished the present age, the machinery invented by Mr Babbage for performing intellectual labour, is entitled to a prominent place. Low as that species of mental exertion undoubtedly is, when the mathematician performs the monotonous round of arithmetical calculations, in which neither the power of combination nor of judgment is called into action; yet we were not prepared to see even these humbler functions placed under the surveillance of wheels and pinions; and hence the first intelligence of a calculating machine has been received with as much incredulity by the wise as by the vulgar.

The object which Mr Babbage had in view in constructing this new machinery, was to produce printed copies of any mathematical tables, *without the possibility of an error existing in a single copy.* This result he proposed to attain solely by ma-equal in speed to the writer. At another trial, *thirty-two* numbers of the same table were calculated in *two minutes and thirty seconds*; and, as these contained *eighty-two* figures, the engine produced *thirty-three* every *minute*. Another time it produced *forty-four* figures in a minute; and, as the machine may be moved uniformly by a weight, this rate of computation may be maintained for any length of time; and it is probable that few writers are able to copy, with equal speed, for many hours together.

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* This paper is [illegible]om different private sources of information, but particularly from [illegible]inted letter on the subject, addressed to Sir Humphry Davy, B[illegible]

Nothwithstanding the defective workmanship in this first machine, its computations are wonderfully accurate; and some very skilful mechanics to whom Mr Babbage has shewn it in confidence, are of opinion that it may be carried to any extent. Its parts, indeed, are few, but frequently repeated; and, though it contains many wheels, yet only a few of them move at the same time, so that its simplicity is greater than that of many machines in common and constant use.

There is one circumstance in the construction of this machine, which is of considerable importance in making larger ones, viz. that, though its wheels are numerous, yet, by a peculiar contrivance, any error produced by accident, or by a slight inaccuracy in one of them, is corrected as soon as it is transmitted to the next, and in such a manner as effectually to prevent any accumulation of small errors from producing a wrong figure in the calculation.

The machine for composing with types, has not yet been actually constructed; but, from several models which Mr Babbage has made, and from numerous experiments which he has tried, there can be no doubt of its complete success *. When put up, the composing machine will contain about 30,000 types, which are put in their places by children; but the person who attends the engine has a method of ascertaining, in less than half an hour, *if any one individual of this number is misplaced.*

As the engine for computing tables by the method of differences, is the only one which Mr Babbage has yet completed, he has, in his letter to Sir Humphry Davy, confined himself to a statement of the powers which that method possesses, and as this statement is [redacted]teresting, we shall give it in his own words.

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* A working-[redacted]the machinery has been recently constructed.

I would, however, premise, that if any one shall be of opinion, notwithstanding all the precautions I have taken and means I have employed to guard against the occurrence of error, that it may still be possible for it to arise, the method of differences enables me to determine its existence. Thus, if proper numbers are placed at the outset in the engine, and if it has composed a page of any kind of table, then by comparing the last number it has set up with that number previously calculated, if they are found to agree, the whole page must be correct: should any disagreement occur, it would scarcely be worth the trouble of looking for its origin, as the shortest plan would be to make the engine recalculate the whole page, and nothing would be lost but a few hours' labour of the moving power.

Of the variety of tables which such an engine could calculate, I shall mention but a few. The tables of powers and products published at the expence of the Board of Longitude, and calculated by Dr Hutton, were solely executed by the method of differences; and other tables of the roots of numbers have been calculated by the same gentleman on similar principles.

As it is not my intention in the present instance to enter into the theory of differences, a field far too wide for the limits of this letter, and which will probably be yet further extended in consequence of the machinery I have contrived, I shall content myself with describing the course pursued in one of the most stupendous monuments of arithmetical calculation which the world has yet produced, and shall point out the mode in which it was conducted, and what share of mental labour would have been saved by the employment of such an engine as I have contrived.

The tables to which I allude are those calculated under the direction of M. Prony by order of the French Government;—a work which will ever reflect the highest credit on the nation which patronized, and on the scientific men who executed it. The tables computed were the following.

1. The natural sines of each 10,000 of the quadrant calculated to twenty-five figures with seven or eight orders of differences.

2. The logarithmic sines of each 100,000 of the quadrant calculated to fourteen decimals with five orders of differences.

3. The logarithm of the ratios of the sines to their arcs of the first 5000 of the 100,000ths of the quadrant calculated to fourteen decimals with three orders of differences.

4. The logarithmic tangents corresponding to the logarithmic sines calculated to the same extent.

5. The logarithms of the ratios of the tangents to their arcs calculated in the same manner as the logarithms of the ratios of the sines to their arcs.

6. The logarithms of numbers from 1 to 10,000 calculated to nineteen decimals.

7. The logarithms of all numbers from 10,000 to 200,000 calculated to fourteen figures with five orders of differences.

Such are the tables which have been calculated, occupying in their present state seventeen large folio volumes. It will be observed, that the trigonometrical tables are adapted to the decimal system, which has not been generally adopted even by the French, and which has not been at all employed in this country. But, notwithstanding this objection, such was the opinion entertained of their value, that a distinguished member of the English Board of Longitude, was not long since commissioned by our Government to make a proposal to the Board of Longitude of France, to print an abridgment of these tables at the joint expence of the two countries; and L. 5000 were named as the sum our Government was willing to advance for this purpose. It is gratifying to record this disinterested offer, so far above those little jealousies which frequently interfere between nations long rivals, and manifesting so sincere a desire to render useful to mankind the best materials of science in whatever country they might be produced. Of the reasons why this proposal was declined by our neighbours, I am at present uninformed; but, from a personal acquaintance with many of the distinguished foreigners to whom it was referred, I am convinced that it was received with the same good feelings as those which dictated it.

I will now endeavour shortly to state the manner in which this enormous mass of computation was executed; one table of which (that of the logarithms of numbers), must contain about eight mi figures.

The calcula ed into three sections. The first section compri athematicians of the highest merit, amongst whom were M. Prony and M. Legendre. These were occupied entirely with the analytical part of the work; they investigated and determined on the formulæ to be employed.

The second section consisted of seven or eight skilful calculators, habituated both to analytical and arithmetical computations. These received the formulæ from the first section, converted them into numbers, and furnished to the third section the proper differences at the stated intervals.

They also received from that section the calculated results, and compared the two sets, which were computed independently for the purpose of verification.

The third section, on whom the most laborious part of the operations devolved, consisted of from sixty to eighty persons, few of them possessing a knowledge of more than the first rules of arithmetic: these received from the second class certain numbers and differences, with which, by additions and subtractions in a prescribed period, they completed the whole of the tables above mentioned.

I will now examine what portion of this labour might be dispensed with, in case it should be deemed advisable to compute these or any similar tables of equal extent by the aid of the engine I have referred to.

In the first place, the labour of the first section would be considerably reduced; because the formulæ used in the great work I have been describing, have already been investigated and published. One person, or at the utmost two, might therefore conduct it.

If the persons composing the second section, instead of delivering the numbers they calculate to the computers of the third section, were to deliver them to the engine, the whole of the remaining operations would be executed by machinery, and it would only be necessary to employ people to copy down as fast as they were able the figures presented to them by the engine. If, however, the contrivances for printing were brought to perfection and employed, even this labour would be unnecessary, and a few superintendants would manage the machine and receive the calculated pages set up in type. Thus, the number of calculators employed, instead of amounting to ninety-six, would be reduced to twelve. This number might, however, be considerably diminished; because when an engine is used, the intervals between the differences calculated by the second section may be greatly enlarged. In the tables of logarithms, M. Prony caused the differences to be calculated at intervals of 200, in order to save the labour of the third section; but as that would now devolve on machinery, which would scarcely move the slower for its additional burthen, the intervals might properly be enlarged to three or four times that quantity. This would cause a considerable diminution in the labour of the second section. If, to this diminution of mental labour we add that which arises from the whole work of the compositor being executed by the machine, and the total suppression of that most annoying of all literary labour, the correction of the errors of the press, I think I am justified in presuming, that if engines were made purposely for this object, and were afterwards useless, the tables could be produced at a much cheaper rate: and of their superior accuracy there could be no doubt. Such engines would, however, be far from useless: containing within themselves the power of generating to an almost unlimited extent, tables whose accuracy would be unrivalled, at an expence comparatively moderate, they would become active agents in reducing the abstract inquiries of geometry to a form and an arrangement adapted to the ordinary purposes of human society.

I should be unwilling to terminate this letter, without noticing another class of tables of the greatest importance, almost the whole of which are capable of being calculated by the method of differences. I refer to all astronomical tables for determining the positions of the sun or planets. It is scarcely necessary to observe, that the constituent parts of these are of the form $a \sin \theta$, where a is a constant quantity, and θ is what is usually called the argument. Viewed in this light, they differ but little from a table of sines, and like it may be computed by the method of differences."

Having thus given our readers a succinct description of the nature and ob[illegible] of Mr Babbage's arithmetical machinery, we have only to [illegible]ope that the British government, or some of thos[illegible]hich it so liberally supports, will afford Mr Ba[illegible] of constructing a large engine, and hold out to him some inducement to withdraw his attention from more alluring studies, and devote it to the completion of an invention which will do honour to his country, and to the age in which we live.

EDINBURGH, *August* 1. 1822.

ART. XV.—*Notice respecting* MR BARLOW'S *Discovery of the Mathematical Laws of Electro-Magnetism.* *

MR BARLOW of the Royal Military Academy, who has so successfully reduced the laws of induced magnetism to mathematical principles, has been equally fortunate in his experiments and investigations on electro-magnetism, having rendered this also a matter of computation.

We are not exactly informed of the apparatus which he employed in these researches; but it appears that the machine itself was on the principle of Dr Hare's Calorimotor, and the other part consisted simply of a rectangle of stout brass wire, each side of which was four feet. One side of this rectangle was open, so as to make the connection with the battery, and the other vertical side was passed through the centre of a table, divided into the several points of the compass, and round which, therefore, a magnetic needle might be placed at any azimuth. The two horizontal sides of the rectangle might be slipped up and down on the vertical wires, whereby the length of the conducting part of the vertical wire might be changed at pleasure; and the distance of the compass itself from the vertical wire, might also, in like manner, be varied *ad libitum*, by merely sliding to and from the centre. By these means, Mr Barlow was enabled first to determine the law of the electro-magnetic action, as it depended upon the distance, the length of the wire remaining constant; and that law he found to be, that the action was inversely as the square of the distance, from every particle of the vertical wire. The distance being now made constant, and the length of the wire varied, he found still the same result, viz. that every particle of the galvanic wire acts on every particle of the magnetic needle, with a power varying inversely as the square of the distance, the power of the battery being maintained at the same degree of intensity; which latter condition was insured, by means of a mechanical contrivance, for lowering gradually the combination of plates, into the vessel containing the dilute acid, and a standard compass fixed opposite the other vertical branch of the conducting rectangle.

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* Our readers will be glad to learn that Mr Barlow is printing a second edition of his "Essay on Magnetic Attractions," which will embrace also the subject of *Electro-magnetism.*—ED.

Having established this point, the needle was now passed successively, at different distances, round the wire, and the law of deviation ascertained, as the same depended upon the azimuth of the needle with respect to the wire, and which law is thus given by the author: "Let Δ represent the deviation of the needle at the north point; and φ the angle of the azimuth from the north, and ∂ the deviation of that azimuth; then

$$\tan \partial = \frac{\cos \varphi}{\cot \Delta \times \sin \varphi.}$$

This formula, although so extremely simple, is sufficient to explain all the peculiarities of action observed between the needle and the wire, by merely attending to the changes of signs which take place in $\cos \varphi$ and $\sin \varphi$ in the four quadrants, and to the distinct cases which follow from considering $\cot \Delta$, as greater than, equal to, or less than unity. In the two latter cases $\tan \partial$ will become infinite, and the needle will revolve on its pivot during its rotation round the wire, which cannot take place if $\cot \Delta$ be greater than unity, that is, if the deviation at the north be less than 45°. In all these cases, and in a long series of computed and observed deviations, Mr Barlow has found the most complete and satisfactory coincidences, and comparing together the experimental and theoretical laws of action, he has drawn the following general conclusions, viz. That every particle of the galvanic fluid in the conducting wire, acts on every particle of the magnetic fluid in a magnetized needle, by a force which varies i[illegible] as the square of the distance; but that this action is [illegible] act nor to repel either pole of the magnetic part [illegible] ntial force, which is reciprocal between the two fluids, and which tends to place the poles of either at right angles to those of the other and to the right line which joins them. By admitting this species of action, the formula above given is immediately deduced, and all the experimental deviations at every azimuth, and at any distance, and with any length of conducting wire, as readily computed; and the coincidences, as above stated, are in every case to the highest degree satisfactory. The same theory is equally applicable to the explanation of the very interesting rotatory experiments of Mr Faraday, and, in short, to every phenomenon that has yet been observed in this new branch of natural philosophy.

ART. XVI.—*Observations on the Natural Family of Plants called Polemoniaceæ; with Descriptions of the Genera belonging to it, and of a Genus improperly referred to that Order by Botanists.* By Mr DAVID DON, Curator of the Lambertian Herbarium.

NOTHING, certainly, has contributed so much to the rapid progress of systematic botany in our days, as the journeys of scientific travellers in distant countries, and the formation of extensive herbariums. The Order *Polemoniaceæ*, established long ago by Professor de Jussieu, in his admirable *Genera Plantarum*, has, however, received little addition or illustration since, either by himself or others. But as the advancement of every science must necessarily be slow and progressive, and, as every addition leads to fresh discoveries, so in botany, every new discovery serves powerfully to illustrate the relative affinities of orders, genera, and even species. The ample advantages I have enjoyed during the period my attention has been directed to this subject, in having the extensive collections of Ruiz and Pavon under my immediate inspection, besides other plants belonging to this order, from the Abbe Cavanilles, have enabled me to render the present essay on the *Polemoniaceæ* much more complete than I could otherwise have made it. The results will, I trust, prove not unacceptable to botanists.

The Order *Polemoniaceæ* is characterised by a regular monopetalous, pentandrous, 5-lobed corolla, by a triple stigma; a

3-celled, 3-valved capsule, with septiferous valves, seeds numerous or definite, attached to a 3-sided central placenta, covered by a thin mucilaginous integument, and furnished with a fleshy albumen; and, lastly, by a straight embryo, with somewhat foliaceous, orbicular or oval cotyledons, frequently longer than the straight, thickest inferior and obtuse radicle. The order most nearly allied to it is *Convolvulaceæ*, which, however, is readily distinguished from it, by having a simple or double stigma; by the septa, instead of proceeding from the centre of the valves of the capsule, being directed to their margins; and the seeds, having no central placenta, being attached to the bottom of the cells. The seeds are very different, being covered by a thin, very fragile, mucilaginous albumen; with a curved embryo, having large, foliaceous, plaited and wrinkled, emarginate cotyledons. The *Polemoniaceæ*, through the genus *Cantua*, has some affinity, likewise, with the *Bignoniaceæ;* but this order has an irregular corolla, a bilamellate stigma, a very different capsule; transverse seeds, destitute of albumen; and, lastly, a foliaceous embryo, with a curved radicle. Externally, the *Plumbagineæ* have some resemblance to the *Polemoniaceæ*, chiefly through the genera *Phlox* and *Plumbago;* but the former order has four or five stigmas, with a single-seeded ovarium attached to the bottom of the calyx. The figure of its embryo corresponds exactly with that of the *Polemoniaceæ;* but it is inverted, and surrounded by a very white farinaceous albumen. Professor Link (*Fl. Portugaise*) has separated Statice and Armeria from *Plumbagineæ*, to constitute a new order, which he has named *Staticinæ*. In this he has been followed by De Candolle and others; but, assuredly, there seems no good reason for such a division: both agreeing exactly in the structure of their seeds and embryo. It is true the *Staticinæ*, have 5 separate styles, and a 5-petalled corolla; but this last character is not always constant, for there are some species of *Statice* with monopetalous corollas. The insertion of stamens, which in *Plumbago* is hypogynous, and epipetalous in *Statice*, is, perhaps, of less importance than is generally admitted; fo[illegible] most cases of epipetalous insertion (as it is erroneously te[illegible] trace the filaments to the base of the corolla; th[illegible] ly united to the petals.

The order P[illegible] sists of six very natural genera, namely, *Polemonium*, *Phlox*, *Cantua*, *Hoitzia*, *Caldasia*, and *Ipomopsis*, for which I have preferred the prior name of *Gilia* of the *Flora Peruviana*. The name *Cantua* being of barbarous origin, and not generally applied by the natives to these plants, led the authors of the *Flora Peruviana* to change it to *Periphragmos*, a name of Greek derivation. There seems, however, no good reason for such a change, and I have therefore followed the great Jussieu in adopting the name of *Cantua*. Names derived from languages, such as the Mexican and Peruvian, which are eminently soft, when cropt of their length, are certainly much less objectionable, than giving to plants the names of men who have no pretensions to a place in the annals of botany;—a practice which has now become too common, and which reflects discredit on the science, as well as on those who impose them. The *Periphragmos fœtidus* of the *Fl. Peruviana*, has been judiciously separated by Willdenow[*] from *Cantua* Juss. under the name of *Vestia*. An attentive examination of this plant, which produced perfect flowers and ripe fruit this season, in the Chelsea Botanic Garden, proves, that it ought to be referred to the *Solaneæ* near to *Cestrum*, with which it agrees in the form and valvular æstivation of its corolla; although it differs also very materially from it, in having a straight embryo. The figure of the fruit, given by the authors of the *Flora Peruviana*, certainly does not belong to it, but distinctly to a species of *Cantua*, and most probably to *C. pyrifolia*, Juss. (*Periphragmos flexuosus*, Fl. Peruv). The erroneous figure in *Flora Peruviana* has probably misled M. de Jussieu [†], to refer this plant to his genus *Cantua*, with which, unless habit, it has scarcely a single character in common. It is, therefore, surprising that M. Kunth [‡] should again unite it to *Cantua*. The same scientific botanist, in the work quoted, has erroneously described the seeds of *Hoitzia* as naked. The three species of *Polemonium* mentioned by M. Kunth [§], will be found, we fear, to constitute but one species, namely, *Polemonium Mexicanum* of Cervantes and Lagasca [‖]. M. Kunth not having seen specimens of the species he has indicated, has been obliged to trust to Willdenow, whose descriptions, especially what relates to the fructification, are extremely incorrect. In forming the following characters of the order and genera, the greater part of the species have been carefully examined. This I think it important to state, as it will shew how far the results are to be depended on.—I shall now proceed to give the character of the order.

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[*] Enum. i. p. 208.

[†] Annales du Museum, xv. p. 341.

[‡] Nov. Gen. &c. iii. p. 161.

[§] Ibid. iii. p. 160.

[‖] Plant. Nov. p. 10.

Nat. Ord. POLEMONIACEÆ, *Juss.*

Calyx tubulosus, 5-fidus aut 5-dentatus.

Corolla rotata v. hypocrateriformis aut sæpiùs infundibuliformis, limbo 5-loba: *lobis* in æstivatione imbricatis.

Stamina 5, corollæ tubo nunc infra nunc supra medium inserta. *Antheræ* sagittatæ, incumbentes, biloculares.

Ovarium simplex, semper triloculare, integrum: *ovulis* numerosis raró definitis. *Stylus* longissimus. *Stigma* triplex: *lobis* linearibus, obtusis.

Capsula 3-locularis, 3-valvis, oblonga, trigona, rariùs globosa: *valvis* margine appositis, intùs medio septiferis, membranaceis aut rariùs (in *Cantuâ*) crassiusculis, crustaceis. *Placenta* trigona, centralis, cujus angulis septa applicata.

Semina convexa, angulata, nuda aut compressa margine membranâ cincta: *Testa* simplex, membranacea, superficie mucilaginosa: *Albumen* carnosum, semini conforme.

Embryo rectus, magnus: *Cotyl.* planæ, orbiculatæ v. ovales, subfoliaceæ: *Radicula* infera, crassiuscula, obtusa, recta, sæpiùs cotyledonibus brevior.

Herbæ *v.* Frutices, *sæpiùs ramosæ.* Folia *alterna aut rariùs* (in *Phlox*) *opposita, exstipulata, indivisa v. pinnatifida.* Flores *terminales, numerosi aut axillares, subsolitarii, in quibusdam bracteati.*

Obs. An ordini huic admittenda parùm mutato charactere *Læselia?* Polemoniaceis in multis accedunt Convolvulaceæ; sed tamen o abundè diversum hæ constituunt, et ab iisdem maxin stigmate simplici v. duplici, capsulæ valvis intus pto iisdem marginibus applicato, placentâ cent ibus albumine parco valdè fragili

mucilaginoso præditis, embryone arcuato, cotyledonibus maxime foliaceis contortuplicatis corrugatisque emarginatis, radiculâ curvatâ iisdem multò breviore.

CHARACTERES GENERUM.

Polemonium, *Linn.*

Calyx campanulatus, 5-fidus. *Corolla* rotata, limbo 5-loba, erecta. *Stamina* in fauce inserta: *filamenta* basi barbata, in annulam contigua et faucem ferè claudentia. *Capsula* subrotunda: *loculis* polyspermis: *valvis* membranaceo-crustaceis. *Semina* oblonga, subtrigona, albumine magno prædita. *Radicula* cotyledonibus duplò longior!

Herbæ *perennes* (*Amer. bor. Europ. et Siber.*) Folia *alterna, impari-pinnata aut rariùs* (*in* P. sibirico) *bipinnata: foliolis integerrimis.* Flores *numerosi in corymbis pluribus paniculatìm digesti, pulchri, cærulei vel rariùs albi.*

Phlox, *Linn.*

Calyx profundè 5-fidus, connivens. *Corolla* hypocrateriformis, limbo explanata, 5-partita: *segmentis* cuneatis. *Stamina* in tubo supra medium inserta, valdè inæqualia. *Capsula* subrotunda: *loculis* monospermis! *valvis* membranaceis. *Semina* oblonga, extùs convexa. *Albumen* magnum. *Radicula* cotyledonibus duplò longior!

Herbæ *perennes, rarò suffrutices* (*Amer. bor.* unâ *ex Asiâ boreali-orientali*) pleriquæ, erectæ. Folia *opposita! integerrima, sessilia.* Flores *terminales, paniculati v. corymbosi, lilacini v. rosei, rariùs rubri, cærulei aut albi.*

Gilia, *Ruiz et Pavon.*
Ipomopsis, *Mich. Smith.*
Cantuæ sp. *Willd. Pursh.*

Calyx tubulosus, 5-fidus. *Corolla* infundibuliformis, in *quibusdam* 5-plò calyce longior, limbo plana, 5-loba: *lobis* latè ovatis. *Stamina* in tubo supra medium inserta. *Capsula* oblonga, subtrigona: *loculis* oligospermis: *valvis* membranaceis. *Semina* angulata. *Albumen* parcum. *Radicula* cotyledonibus brevior.

Herbæ *ramosissimæ* (*Amer. calidior.*) *radice annuâ vel bienni.* Folia *alterna, profundè pinnatifida: segmentis longo-linearibus, integerrimis, rariùs incisis.* Flores *numerosi, pedun-*

culati, subsolitarii aut rariùs glomerati, parvi, purpurei aut cærulei, in eleganti *et* aggregatâ *magni, speciosi, coccinei.*

Hùc referendæ sunt: Gilia laciniata *Fl. Peruv.* 2. *p.* 17. *t.* 123. Ipomopsis elegans, *Mich. Amer.*, *Smith Exot. Bot. t.* 13. quæ Polemonium rubrum *Linn.*, et Cantua coronopifolia *Willd.* Ipomopsis inconspicua *Smith loc. cit. t.* 14., et Cantua aggregata *Pursh. Am. Sept.* 1. *p.* 147.

Obs. Extùs simillima Giliæ eleganti est Ipomæa Quamoclit. *Linn.*, cui tamen præter stigma capitatum simplex, characteres omninò diversi ad Convolvulaceas spectantes.

CALDASIA, *Willd.*

BONPLANDIA, *Cavan.*

Calyx tubulosus, 5-dentatus, corollæ tubum æquans. *Corolla* infundibuliformis, limbo 5-loba: *lobis* cuneatis, emarginatis. *Stamina* in medio tubi inserta. *Capsula* oblonga, trigona: *loculis* monospermis! *valvis* membranaceis. *Semina* oblonga, subtrigona. *Albumen* parcum. *Radicula* cotyledonibus vix brevior.

Herba (*Mexicana*) *annua.* Caules *erecti, ramosi.* Folia *alterna, simplicia, elliptica, acuminata, serrata.* Flores *axillares, gemini, pedunculati, purpurei.*

HOITZIA, *Juss.*

CANTUÆ sp. *Willd.*

Calyx tubulosus, aridus, 5-fidus laciniis pungentibus, extùs bracteis 5! lanceolatis pungenti-mucronatis munitus. *Corolla* infundibuliformis, calyce triplò longior, limbo 5-loba: *lobis* latè obovatis, integris. *Capsula* trigona: *loculis* oligospermis: *valvis* membranaceo-crustaceis. *Semina* compressa, suborbiculata, membranâ angustissimâ cincta. *Albumen* parcum. *Radicula* cotyledonibus subæqualis.

Suffrutices (*Mexicani*) *rigidi, ramosi, ramis teretibus pubescentibus villosisve.* Folia *alterna, ovata v. lanceolata, mucronata, hirsuta, argutè serrata, reticulato-venosa, basi cuneata, brevissimè petiolata.* Flores *solitarii aut gemini, axillares, pedicellati, magni, speciosi, coccinei aut violacei.*

Obs. Ad [illegible] pertinent sequentes species, scilicet, H. coccinea *Cava*[illegible]., cœrulea *ejusd. t.* 366., glandulosa *ibid.* t. 367. [illegible] verisimilitèr H. Cervantesii *Kunth* *in Nov. Gen. &c.* 3. *p.* 164, aristata *Kunth in loc. cit. p.* 164., conglomerata *ejusd. p.* 165.

CANTUA, *Juss.*

PERIPHRAGMOS, *Ruiz et Pavon.*

Calyx tubulosus, 5-dentatus, sæpiùs utroque lateri usque medium fissus. *Corolla* infundibuliformis, calyce triplò longior, limbo 5-loba: *lobis* obcordatis. *Stamina* in tubo juxta basin inserta. *Capsula* oblonga, trigona, *sulcis* 3, septis oppositis exarata: *loculis* polyspermis: *valvis* coriaceo-crustaceis. *Semina* ovalia, compressa, alâ oblongâ membranaceâ cincta! *Albumen* pa cum. *Radicula* cotyledonibus brevior.

Frutices (*Peruviani*) *erecti, ramosi,* Lycii *facie.* Folia *alterna, petiolata, elliptica, acuminata vel cuneato-oblonga, integerrima v. serrata, aut rariùs pinnatifido-sinuata, subtus cum junioribus pubescentia.* Flores *terminales, corymbosi v. subsolitarii* (*ut in* C. ovata *Cavan. icon. t.* 363.), *albi v. purpurei, in* C. buxifoliâ *maximi, speciosissimi.*

Characteres specierum.

1. *C. pyrifolia,* foliis ellipticis acuminatis integerrimis, corymbis densifloris, pedunculis pubescentibus, staminibus corollâ curvatâ duplò longioribus.

C. pyrifolia, *Juss. in Ann. Mus. Paris.* 3. *p.* 117. *t.* 7. *Lam. Encycl.* 1. *p.* 603. *ejusd. Illustr.* 1. *t.* 106. *f.* 1. *Willd. Sp. Pl.* 1. *p.* 878. *Kunth in Nov. Gen. &c.* 3. *p.* 161.

Periphragmos flexuosus, *Ruiz et Pavon Fl. Peruv.* 2. *p.* 17. *t.* 131.

Hab. in Peruviâ, ♄. *Turu* ab Indigenis nominatur.

2. *C. theæfolia,* foliis ovato-lanceolatis obtusè acuminatis serratis, corymbis tenuifloris, pedunculis glabris, staminibus corollâ curvatâ brevioribus.

Periphragmos, sp. nova. *Pavon MSS.*

Hab. in Peruviâ, ♄.

Frutex præcedentis facie; sed distinctissima foliis longioribus serratis obtusè acuminatis, corymbis paucifloris, pedunculis glabris, corollis duplò majoribus staminibus inclusis, capsulis longioribus, seminibus multò majoribus.

3. *C. quercifolia,* foliis oblongis sinuato-pinnatifidis acuminatis, corymbis densifloris, pedunculis glanduloso-pilosis, staminibus corollam curvatam longè superantibus.

C. quercifolia, *Juss. in Ann. Mus. Paris.* 3. *p.* 118. *Kunth in Nov. Gen. &c.* 3. *p.* 161.

Hab. in Peruviâ, ♄. *Pipiso* ab Indigenis dicta.

4. *C. buxifolia*, foliis cuneato-oblongis mucronulatis integerrimis, corymbis paucifloris, pedunculis tomentosis, staminibus corollâ rectâ brevioribus.

Cantua buxifolia, *Juss. in loc. cit.* 3. *p.* 118. *t.* 8. *Lam. Enc.* 1. *p.* 603. *ibid. Illustr.* 1. *t.* 106. *f.* 2. *Willd. Sp. Pl.* 1. *p.* 878.

Cantua tomentosa, *Cavan. Icon.* 4. *p.* 43. *t.* 364.

Periphragmos dependens, *Ruiz et Pavon Fl. Peruv.* 2. *p.* 18. *t.* 133.

Hab. in Peruviâ, ♄. *Ccantu* ab Indigenis dicitur.

Obs. *Frutex* pulcherrimus cujus flores admodum speciosi templa adornanda in festis usitati sunt. *Folia* et *Lignum* hujus ut etiamque cæterarum specierum colorem elegantèr luteum quando contrita aut masticata præbent, ideoque ad lintea inficienda forsan utilia essent.

5. *C. ovata*, foliis obovatis mucronatis integris rarius incisodentatisve, pedunculis solitariis unifloris glabris, staminibus corollâ rectâ longioribus.

Cantua ovata, *Cavan. Icon.* 4. *p.* 43. *t.* 363. *Juss. in Ann. Mus. Paris.* 3. *p.* 118.

Periphragmos uniflorus, *Ruiz et Pavon Fl. Peruv.* 2. *p.* 18.

Hab. in Peruviâ, ♄. *Ccantuttica*, id est *Ccantu Flos*, vernaculè nuncupatur.

I shall now conclude this essay, by adding a full description of the genus *Vestia*, hitherto so improperly referred to the *Polemoniaceæ*, and to whose distinguishing marks so little attention has been paid.

Nat. Ord. SOLANEÆ, *Juss.*
PENTANDRIA MONOGYNIA, *Syst. Linn.*
VESTIA, *Willd.*
CANTUÆ sp. *Juss. Kunth.*
PERIPHRAGMI sp. *Ruiz et Pavon.*

Calyx ca [illegible] 5-dentatus: *dentibus* latè ovatis, apice subulato-cu [illegible] *lla* infundibuliformis, recta, calyce triplò longior: *limbo* 5-lobo, æquali, erecto-patente: *lobis* ovatis, subacutis, in æstivatione valvatis. *Stamina* 5, exserta, erecta, in tubo infra medium inserta: *filamenta* longissima, filiformia, glabra, basi fasciculo pilorum barbata: *antheræ* subrotundæ, nudæ, bilobæ, biloculares: *loculis* parallelis, æqualibus, fertilibus, bivalvibus, rimâ longitudinali lateraliter dehiscentes, polline granuloso albo turgidæ. *Pistillum* 1: *ovarium* globosum, glabrum, membranâ cupulatâ basi cinctum: *stylus* filiformis, rectus, staminibus longior, glaber: *stigma* incrassatum, integrum, obtusum. *Capsula* subrotunda, sulcis 4 cruciatis exarata, 2-locularis, 4-valvis, polysperma: *valvis* ellipticis, coriaceis ab apice dehiscentibus. *Placentæ* 2, magnæ, convexæ, carnosæ, succulentæ, lacunosæ, seminibus creberrimè tectæ, dissepimento membranaceo parallelo applicatæ, ab valvis liberæ. *Semina* ovalia vel oblonga, nuda, subcompressa, glabra: *testa* simplex, membranacea, rugosa, atrofusca: *albumen* magnum, carnosum, album. *Embryo* rectus! albus, magnus, in albumine centralis: *cotyl.* orbiculatæ, compressæ: *radicula* crassiuscula, teres, recta, cotyledonibus duplò longior, infera, obtusa.

Frutex (*Chilensis*) *erectus, ramosus, facie* Cestri *aut* Lycii, *glaber.* Folia *alterna, obovata vel elliptica, petiolata, integerrima, coriacea, glabra, nitida, intensè viridia, basi attenuata.* Flores *sparsi, terminales, solitarii, magni, flavi, pedunculati, nutantes.*

Obs. Species una tantum adhuc nota, viz. Vestia lycioides *Willd. enum.* 1. *p.* 208., quæ Cantua ligustrifolia *Juss. in Ann. Mus. Paris.* 3. *p.* 118., et Periphragmos fœtidus *Ruiz et Pavon, Fl. Peruv.* 2. *p.* 17. *t.* 132; sed cum capsulâ *Cantuæ* veræ speciei, et forsan *Cantuæ pyrifoliæ.* Tota planta quando contrita odorem gravidum et fœtidum ferè Melianthii spirat. *Folia* masticata ex observationibus clarissim. auctorum *Floræ Peruvianæ* colorem luteum præbent; etiamque cum tota planta ab Indis regni Chilensis, quibus *Guevillguevill* nuncupatur, ad ardores sanguinis mitigandos in clysteribus vel decocto, et ad febres tabificas *Congo* aut *Chavalongo*, et dysenteriam depellendas, in decocto aut infuso usurpantur.

ART. XVII.—*On the Method of Minimum Squares, employed in the Reduction of Experiments, being a Translation of the Appendix to an Essay of Legendre's, entitled,* " Nouvelles Methodes pour la Determination des Orbites des Cometes," *with Remarks.* By GEORGE HARVEY, Esq. Member of the London Astronomical Society.

A Translation of an Essay by Legendre, on the subject of *Minimum Squares*, may not be unacceptable to such of the readers of the Edinburgh Journal, as may not have had an opportunity of consulting the able memoirs which have appeared on this very important subject, in the writings of some of the Continental mathematicians. I have selected the paper of Legendre; because that distinguished geometer was the first who added to the equations of condition employed by Euler, in his paper on the inequalities of Jupiter and Saturn, and by Mayer, in his Essay on the Moon's Libration,—an equation which should involve the additional condition of making the sums of the squares of the errors of observation a *minimum*. In another paper, I shall give the very able demonstration which Laplace has afforded, in his *Theorie Analytique des Probabilités*, that the result which Legendre's principle affords, is the best which the conditions of any problem can require; since the result of the investigation leaves the *least possible* error, and, at the same time, points out the limits within which its aberrations are confined.

To the experimental philosopher, the method of *minimum squares* will be frequently found of singular utility and value. In the varied departments of natural philosophy, in practical astronomy, and, indeed, in every inquiry where the conditions to be determined are exhibited under the form of algebraical coefficients, it will fix the mean position or value of the object of research, with a degree of certainty and precision not to be attained by any other method.

The essay of Legendre here alluded to, was published originally as an [illegible]dix to the work which appeared in 1805, and entitled *N* [illegible] *odes pour la Determination des Orbites des Comet* [illegible] listinguished for the same beautiful refinement, th [illegible] the other productions of this celebrated man. Every mathematician acquainted with the labours of Legendre, must have had ample reason for admiring the purity, elegance and fertility, which almost every page of his writings display. As a proof of his powers as a geometrician, we need only refer to his celebrated *Elemens de Geometrie;* a work which has long been the admiration of Europe, and which will, doubtless, continue to be admired, as long as a correct geometrical taste shall prevail *. Of his attainments as an analyst, his large work, entitled *Exercises de Calcul Integral*, bears the most ample proofs. This latter performance, indeed, from its profound and difficult nature, has not met with the wide circulation which has distinguished his Elements of Geometry; but for nice analytical skill, for elegance, for beautiful and varied invention, and all the qualities which distinguish the higher and more refined walks of analytical inquiry, it is unquestionably unrivalled.

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* I am happy to find, that the wide circulation of Legendre's *Elemens de Geometrie*, is likely to be still farther increased by the intended translation edited by Dr Brewster. It will extend the fame of Legendre, and, at the same time, confer an essential benefit on the young mathematicians of this island; and particularly, indeed, on a class of men of no little importance to the country, and who have contributed in a large degree to the successful advancement of its arts, its sciences, and its wealth. I allude to those self-taught men, who, pursuing humble occupations in towns, or living in villages and other retired spots, pass a life of labour in virtuous obscurity, filling up the intervals of their leisure with mathematical and philosophical pursuits. Cornwall, in particular, has long been distinguished for an intelligent class of this kind, arising, most probably, from the practical utility of such information in the operations of mining. In a recent geological tour through that country, I went from the Land's End in a direction nearly parallel to Whitesand Bay to Cape Cornwall, and from thence to the little town of St Just, which is situated in the neighbourhood of a mining district, famous for the rich variety of its minerals, and for the bold and stupendous character of its rocky scenery. In this obscure spot, and which appeared to present but few facilities for the attainment of science, I called on a Mr John Davy, whose ingenious mathematical investigations in that useful publication, The Ladies' Diary, had long attracted my attention. In a little cottage I found this worthy man, surrounded with a numerous and healthy family, and in whose countenances I could read contentment and peace. His scanty library consisted of a few choice English mathematical writers, and whose state plainly proved, how often the volumes had been examined and read. His knowledge, I found, to be far above the elements of science. Of the Foreign writers, indeed, he had but the slightest knowledge; and appeared considerably surprised when I mentioned to him a few of their celebrated works, and what the powers of the modern analysis had achieved. He mentioned the names of Emerson, Simpson and Maclaurin, with enthusiasm and respect; and I shall never forget the honest pride with which he shewed a complimentary letter that he once received from the venerable Dr Hutton. To such a man, a work in a foreign tongue presents a great, and, I have known in more instances than one, an insuperable obstacle; and it is to this very numerous class, that the translation of Legendre's Geometry will be so peculiarly acceptable. It is from this class that unnumbered improvements in the sciences and the arts have proceeded. Need I mention, in confirmation of this, the names of Simpson, of Arkwright, of Brindly, and of Hutton.

In any investigation relating to the subject of minimum squares, the name of Cotes must not be forgotten. With this distinguished man the subject may be said to have originated, since he was the first who allowed each observation to have an influence on the object of inquiry, dependent on its known value. In this, as indeed in many other instances, the fine originality of Cotes' mind was clearly displayed. His merit must have, indeed, been great, when Newton said of him, "If Cotes had lived, we had known something."

On the Method of Minimum Squares.

In most of those investigations whose object is to deduce from experimental observations, the most accurate results they are capable of affording, we are generally conducted to a system of equations of the form

$$E = a + bx + cy + fz + \&c.,$$

in which a, b, c, f, &c. are known coefficients, varying from one equation to the other, and x, y, z, &c. unknown quantities, whose values it is necessary to determine by the condition, that the value of E is reduced either to Zero, or to a very small quantity in each equation.

If we had as many equations as unknown quantities x, y, z, &c., there would be no difficulty in determining the values of the latter, in such a manner as to render the errors denoted by E, absolutely null. But it most frequently happens, that the number of equations exceed that of the unknown quantities; and hence it becomes impossible to annihilate all the errors connected with the investigation.

As this is a condition which appertains to the greater part of those physical and astronomical problems, whose object is the determination [illegible] rtant elements, it is necessary to exercise some dis [illegible] stribution of the errors, and not to expect that a [illegible] employed should conduct precisely to the sam [illegible] above all, we must endeavour to confine the extreme errors within the least possible limits, without attending to the nature of their signs.

Of all the principles that can be proposed for the attainment of this object, there is none more general, more exact, or of an easier application, than that which consists in making the sum of the squares of the errors a *minimum*. By this method, there is established among the errors a kind of equilibrium; and which, by preventing the extremes from prevailing, renders it very proper for making known the state of the system which approximates the nearest to the truth.

The sum of the squares of the errors E^2, E'^2, E''^2, &c. being

$$\begin{aligned} &(a + b x + c y + f z + \&c.)^2 \\ &+ (a' + b'x + c'y + f'z + \&c.)^2 \\ &+ (a'' + b''x + c''y + f''z + \&c.)^2 \\ &+ \&c.; \end{aligned}$$

if we endeavour to obtain its *minimum*, by making x alone to vary, we shall have the equation

$$0 = \int a b + x \int b^2 + y \int b c + z \int b f + \&c.,$$

in which the sum of the similar products $a\,b$, $a'\,b'$, $a''\,b''$, &c. is denoted by $\int a\,b$, the sum of the squares of the co-efficients of x; that is to say, $b^2 + b'^2 + b''^2 +$ &c. by $\int b^2$, and so on.

The *minimum* with relation to y, will give similarly,

$$0 = \int a c + x \int b c + y \int c^2 + z \int f c + \&c.,$$

and the *minimum* with relation to z,

$$0 = \int a f + x \int b f + y \int c f + z \int f^2 + \&c.,$$

where we perceive that the same co-efficients $\int b\,c$, $\int b\,f$, &c. are common to two equations,—a circumstance which facilitates the operations of the calculus.

In general, *if it be required to form the equation of* minimum *with respect to one of the unknown quantities, it will be necessary to multiply all the terms of the proposed equation by the co-efficient of the unknown quantity in this equation, taken with its proper sign, and then to find the sum of all these products.*

In this manner, we shall obtain as many equations of *minimum*, as there are unknown quantities, and these equations it will be necessary to resolve by the ordinary methods. But we shall find it necessary to abridge the whole of the computation, by admitting into each operation, only as many figures, either whole or fractional, as the degree of approximation of which the question is susceptible, may require.

If it were possible to satisfy all the equations of condition, by rendering each of the errors *null*, we should equally obtain this result by the equations of *minimum*: for, if, after having found the values of x, y, z, &c. which render the errors E E', &c. zero, we cause x, y, z, &c. to vary respectively by ∂x, ∂y, ∂z, &c., it is evident that E^2, which was zero, will become, by this variation, $(a \partial x + b \partial y + c \partial z, \&c.)^2$. A similar consequence will follow for E'^2, E''^2, &c. Hence it is evident, that the sum of the squares of the errors will have for its variation a quantity of the second order, with relation to ∂x, ∂y, &c.,—a principle which perfectly accords with the nature of the *minimum.*

If, after having determined all the unknown quantities x, y, z, &c., we substitute their values in the proposed equations, we shall obtain the different errors E, E' E'', &c. belonging to the system, and which cannot be reduced, without augmenting the sum of their squares. If, among these errors, some are judged to be too considerable to be admitted, the equations which have produced them should be rejected, as resulting from experiments of too defective a kind; and we shall determine the value of the unknown quantities, by means of the remaining equations, which will then present errors of a much smaller kind. And, it is to be observed, that, though we reject the equations which have produced these errors, we shall not be obliged to retrace all the steps of the computation; for, as the equations of *minimum* are formed by the addition of the products belonging to each of the proposed equations, it will be only necessary to remove from that addition those products produced by the equations which have led to the errors.

The rule by which we take the mean between the results of different observations, is but a very simple consequence of our general met [illegible] nd which we shall term the METHOD OF MINIMUM SQUA [illegible]

If expe [illegible] different values a', a'', a''', &c. for a certain qua [illegible] of the squares of the errors will be

$$[illegible] -x)^2 + (a'''-x)^2 + \&c.;$$

and, by making this sum equal to a *minimum*, we shall have

$$0 = (a'-x) + (a''-x) + (a'''-x) + \&c.;$$

and from which results,

$$x = \frac{a' + a'' + a'' + \&c.}{n},$$

n being the number of observations.

In like manner, if, in order to determine the position of a point in space, we have found by a first experiment the co-ordinates a', b', c'; by a second, the co-ordinates a'', b'', c'', &c., and so on, and that we regard x, y, z, as the true co-ordinates of the same point; then the error of the first experiment will be the distance of the point (a', b', c') from the point (x, y, z); the square of this distance is

$$(a'-x)^2+(b'-y)^2+(c'-z)^2;$$

and the sum of the like squares being equal to a *minimum*, we deduce from it three equations, which give

$$x = \int\frac{a}{n}, y = \int\frac{b}{n}, z = \int\frac{c}{n},$$

n being the number of points given by experiment. These formulæ are the same as those by which we find the common centre of gravity of several equal masses, given in position; and from which we perceive that the centre of gravity of any body whatever possesses this general property:

If we divide the mass of a body into equal molecules, and so small that they may be considered as points, the sum of the squares of the distances of these molecules from the centre of gravity will be a minimum.

We perceive, therefore, that the method of minimum squares discloses a kind of centre around which the results furnished by experiment arrange themselves, so that they may be removed from it in the least possible degree. The application we are about to make of it to the measurement of the meridian, will unfold its simplicity and fertility in a clear point of view.

Application to the Measurement of Degrees of the Meridian.

Let us suppose the terrestrial meridian to be any ellipsis whose axes are related to each other as 1 to $1+\alpha$; that D designates the length of the 45th degree, and S that of the arc contained between the two latitudes L and L′; then by known formulæ, and by expressing L′—L in degrees, we shall have

$$S = D(L'-L) - \frac{3}{2}\alpha D \cdot \frac{180}{\pi} \sin(L'-L)\cos(L'+L);$$

from which results

$$L'-L = \frac{S}{D} + \frac{3}{2}\alpha \cdot \frac{180}{\pi} \sin(L'-L)\cos(L'+L).$$

As the 45th degree is about 28500 modules of two toises each, we may make $\frac{1}{D} = \frac{1+\varepsilon}{28500}$, ε being a very small fraction, and we shall have

$$L'-L = \frac{S}{28500} + \varepsilon \cdot \frac{S}{28500} + \alpha \cdot \frac{270}{\pi} \sin(L'-L)\cos(L'+L) \ldots (a);$$

an equation which will furnish the relation between α and ε for every arc whose length is given, and the latitude of whose extremities are known.

In the following table is presented the lengths of the different arcs of the meridian of France, and the latitudes of the parallels which separate them, such as they result from the operations performed by the celebrated astronomers DELAMBRE and MECHAIN.

Place of observation.	Its latitude.	Arcs of the Meridian expressed in modules.	L′—L	L′+L
Dunkirk - - -	51° 2′ 10″.50			
Pantheon at Paris	48 50 49.75	DP 62472.59	2° 11′ 20″.75	99° 53′ 0″
Evaux - - -	46 10 42.50	PE 76145.74	2 40 7.25	95 1 32
Carcassonne - -	43 12 54.40	EC 84424.55	2 57 48.10	89 23 37
Mountjouy - -	41 21 44.80	CM 52749.48	1 51 9.60	84 34 39

We have therefore four arcs, whose measures being substituted successively in the equations (a), will furnish four equations between α and ε. But as these four equations cannot be satisfied all at once, we shall suppose that they exist, by attributing a certain error to the latitude of each place; and for that purpose assume E^{I}, E^{II}, &c. as the additive corrections necessary to be made to the latitudes of Dunkirk, the Pantheon, &c. These errors entering only into the first member of each equation, are too [illegible] ct the term multiplied by α in the second membe[illegible] g therefore are the equations resulting from the four arcs measured in the operations of the meridian:

$$\begin{aligned}
E^{I} - E^{II} &= 0.002923 + \beta\,(2.192) - \alpha\,(0.563)\\
E^{II} - E^{III} &= 0.003100 + \beta\,(2.672) - \alpha\,(0.351)\\
E^{III} - E^{IV} &= -0.001096 + \beta\,(2.962) + \alpha\,(0.047)\\
E^{IV} - E^{V} &= -0.001808 + \beta\,(1.851) + \alpha\,(0.263)
\end{aligned}$$

As it is necessary to consider the errors separately, we shall regard the error E^{III} as a new unknown, and we shall have the five following equations:

$$\begin{aligned}
E^{I} &= E^{III} + 0.006023 + \beta\,(4.864) - \alpha\,(0.914)\\
E^{II} &= E^{III} + 0.003100 + \beta\,(2.672) - \alpha\,(0.351)\\
E^{III} &= E^{III} && (b)\\
E^{IV} &= E^{III} + 0.001096 - \beta\,(2.962) - \alpha\,(0.047)\\
E^{V} &= E^{III} + 0.002904 - \beta\,(4.813) - \alpha\,(0.310)
\end{aligned}$$

We must now endeavour to make the sum of the squares of these five errors a *minimum*, and which condition expressed with relation to the unknown function E^{III}, all of whose co-efficients are unity, will furnish by the addition of all the equations

$$0 = 5\,E^{III} + 0.013123 - \beta\,(0.239) - \alpha\,(1.622)$$

whence

$$E^{III} = -0.002625 + \beta\,(0.048) + \alpha\,(0.324).$$

By substituting this value in the equations denoted by (*b*), we shall have

$$\begin{aligned}
E^{I} &= 0.003398 + \beta\,(4.912) - \alpha\,(0.590)\\
E^{II} &= 0.000475 + \beta\,(2.720) - \alpha\,(0.027)\\
E^{III} &= -0.002625 + \beta\,(0.048) + \alpha\,(0.324) && (c)\\
E^{IV} &= -0.001529 - \beta\,(2.914) + \alpha\,(0.277)\\
E^{V} &= 0.000279 - \beta\,(4.765) + \alpha\,(0.014)
\end{aligned}$$

In order to express afterwards the condition of the *minimum* with relation to β, it will be requisite to multiply the first equation by 4.912 the co-efficient of β; the second by 2.720; the third by 0.048; the fourth by $-$ 2.914; the fifth by $-$ 4.765, and make the sum of all the products equal to zero. And, by operating similarly with respect to α, we shall have the two following equations:

$$\begin{aligned}
0 &= 0.020983 + \beta\,(62.726) - \alpha\,(3.830) && (d)\\
0 &= -0.003287 - \beta\,(3.830) + \alpha\,(0{\cdot}531)
\end{aligned}$$

from which we deduce $\alpha = 0.0675$, and $\beta = 0.0000778$,

and therefore the compression $\alpha = \frac{1}{148}$,

and the 45th degree $D = \frac{28500}{1+\beta} = 28497.78$.

The compression determined by the length of the pendulum and by certain astronomical phenomena, is but $\frac{1}{320}$, and the 45th degree, such as we have deduced from the comparison of the measurements made in France with those of Peru, is 28504.10. The absolute determination of the metre is founded on this last result; and ought to be diminished by about its 4500th part, if we consider only the measurements executed in France; but the compression $\frac{1}{148}$ accords so little with that which we have deduced from other phenomena, that it does not permit us to adopt this last result.

The values found for α and β, determine the ellipsis, which satisfies as exactly as is possible the measurement of the arc of the meridian comprised between Dunkirk and Barcelona. This ellipsis is flattened in a much greater degree than that which corresponds to the general figure of the earth; and supposes errors in the observed latitudes, which may be determined by substituting the values found for α and β, in the expressions for E^{I}, E^{II}, &c.: We shall find by reducing these errors into seconds

$$E^{I} = -0''73,\ E^{II} = 1''83,\ E^{III} = 1''55,\ E^{IV} = 0''.42,\ E^{V} = 0''.03.$$ *

The greatest of these errors does not amount to 2″, and the mean, without regarding the signs, is but 0″91.

If, instead of determining the two quantities α and β, which correspond to the absolute minimum, we begin by making the quantity α equal to the known compression $\frac{1}{320}$, the equations (*c*) will become

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* I have not a [illegible]s of E^{II}, E^{III}, &c. in the text; but I find $E^{II} = 1''.82$, $E^{III} =$ [illegible] 91; the mean error, however, is not affected. H.

$$\begin{aligned}
E^{I} &= 0.001554+\varsigma\,(4.912)\\
E^{II} &= 0.000391+\varsigma\,(2.720)\\
E^{III} &= -0.001612+\varsigma\,(0.048)\\
E^{IV} &= -0.000663-\varsigma\,(2.914)\\
E^{V} &= 0.000323-\varsigma\,(4.765)
\end{aligned}$$

and we shall have for the equation of the *minimum*,

$$0=0.009010+\varsigma\,(62.726),$$

from which results $\varsigma=-0.0001436$ *; and hence the

$$\text{45th degree}=28500\,(1-\varsigma)=28504.09;$$

which sufficiently accords with the adopted determination; but then the errors E^{I}, E^{II}, &c. expressed in seconds, become

$E^{I}=3''06$, $E^{II}=0''00$, $E^{III}=-5''83$, $E^{IV}=-0''88$, $E^{V}=3''62$.

These errors are greater than they were in the case of the absolute minimum; the greatest of them falls on the latitude of Evaux, and the least, which is zero, on that of the Pantheon.

Finally, the anomalies in the latitudes, which undoubtedly ought not to be attributed to the observations, depend probably on the local attractions which act so irregularly on the plumb-line. A defect of homogeneity in the strata near the point where the latitude is observed, is sufficient to occasion it; and the same circumstance which causes the apparent zenith to approach the south or the north, may also cause it to deviate some seconds towards the west, which explains the inequalities we have also observed in the azimuths.

From the anomalies it results, that the length of the arcs of the meridian is less proper for the determination of an universal measure than that of the pendulum; and it is not surprising that some observers, in other respects very exact, have not agreed in the measures they have taken of the degrees of the meridian; since, in consequence of local attractions, the latitudes of two places equally distant from the equator, may differ from each other by several seconds.

PLYMOUTH,
29th July 1822.

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* I find for the equation of the minimum $0=0.009014$, and $\varsigma=-0.0001437$; also the error $E^{I}=3''.05$, and $E^{V}=3''.63$. H.

ART. XVIII.—*On the Methods of separating Lime from Magnesia.* By C. DAUBENY, M. D. M. G. S. (Concluded from p. 123.)

I INFER, therefore, that the plan proposed by Dr Wollaston for ascertaining the presence of magnesia, may, under proper management, be safely extended to the determination of its quantity, where no other substance is present; but it was still a question, whether the remark held good, when any considerable portion of a salt of lime had been originally present.

That, under the latter circumstances, the precipitate afforded by phosphate of soda would be somewhat less than what could be obtained from the same quantity of the magnesian salt unmixed, seemed a necessary consequence of what I had already ascertained as to the precipitation of a portion of that base by carbonate of ammonia, when lime was present; but it was also possible, that as a certain portion of the latter earth escaped the action of the re-agent, and remained in the supernatant liquor, the affinity of the magnesia for it might interfere with the effect of the phosphate of soda upon that base.

It was impossible to try this experiment fairly in the case of sulphate of magnesia, as the sulphate of lime is too sparingly soluble, to be employed in sufficient quantity, and the nitrate or muriate would decompose the magnesian salt. I therefore resolved to ascertain what proportion of a given weight of magnesia could be recovered by the application of phosphate of soda to a solution of the earth in muriatic acid, when a definite quantity of muriate of lime was present, compared to what could be precipitated by the same re-agent, when the magnesia was employed unmixed.

I made several experiments to this effect, but I believe I can best depend on the four following:

	Per Cent.
1. Of 10 gr. of Magnesia recently calcined, I recovered, by means of Phosphate of Soda, - - - -	94.5
2. Of ditto with 10 gr. of Lime, - - -	85.2
3. 10 gr. of Carbonate of Magnesia, - -	80.5
4. Ditto w[illegible]bonate of Lime, - -	77.6

From the [illegible] in these and other attempts to recover the q[illegible] which had appeared to have been present, I might have drawn a conclusion unfavourable to the use of phosphate of soda as a precipitant of magnesia, had it not been for the experiments before detailed; but as these leave no doubt on this head, I am disposed to attribute the apparent error to causes which it may be worth while briefly to mention.

The first of these was the presence of a portion of carbonaceous matter * in the carbonate of ammonia employed, which being retained in chemical combination by the triple phosphate, or at least carried down along with it, proved the means of decomposing a portion of the phosphoric acid, when the precipitate was heated with the view of reducing it to the state of a simple phosphate of magnesia.

The second was, I apprehend, my applying the phosphate in too diluted a condition to act readily on the ammoniaco-magnesian carbonate, owing to which circumstance, a small portion of magnesia remained in solution, as was proved by the addition of pure potash, which rendered the liquor turbid, after the phosphate of soda had ceased to act, although the same re-agent, in a solution to which phosphate of soda had been applied in a more concentrated form, produced, as we have seen, no effect whatever.

But the principal cause of this apparent variation in the results, is the uncertain composition of carbonate of magnesia, which, as Berzelius has shewn, retains a portion of its water, even after it has been exposed to an high temperature for a considerable time †. Hence, the real quantity of magnesia employed, was, in these cases, always below that at which I had estimated it.

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* Carbonate of ammonia has such an affinity for carbonaceous matter, in certain states, that, even when repeatedly sublimed, it seems to carry up some along with it. Hence, in my experiments, whenever an impure carbonate of ammonia was employed, I always found a blackish matter in my solutions; and Dr Kidd informs me, that he has found, that, in passing nitrous acid gas through a solution of ammonia, which he had considered tolerably well purified, a discoloration was produced, which he can only attribute to the action of the nitrous acid upon the bituminous or carbonaceous matter disengaged during the neutralisation of the ammonia.

† See Berzelius's paper "On some Compounds which depend on Weak Affinities," in this *Journal*, vol. i. p. 63, &c.

These circumstances may serve to explain the loss experienced in those cases where the earth was employed unmixed; but the fact, that whenever the same substance was made use of in conjunction with lime, my precipitate fell short of that obtained when the magnesian salt was employed alone, tends to confirm the inference deduced from the foregoing experiments, viz. That a portion of magnesia is thrown down by carbonate of ammonia, when lime happens to be present.

This was further proved by the following experiments, framed under the idea of obviating the uncertainty which the variable composition of the carbonate of magnesia was calculated to produce.

One ounce of muriate of lime, which had previously been found to contain 11.4 grains of base, was mixed with an equal quantity of a solution of muriate of magnesia, which, from the precipitate afforded by phosphate of soda, appeared to contain 5.76 grains of earth. Of these 11.4 grains of lime, carbonate of ammonia threw down 10.8, when no magnesia was present; but the same re-agent added to the mixed solutions, gave a precipitate which indicated no more than 10.2, taking the mean of three experiments.

Yet, although the addition of oxalate of ammonia clearly proved that lime was present in the supernatant liquor, it appeared, on the other hand, that a portion of magnesia had been precipitated; for the powder being redissolved in an acid, and treated as before, gave evident marks of the presence of the latter earth.

I also found, that the quantity of magnesia, indicated by the precipitate, which phosphate of soda afforded was less by 0.2 gr. than when no lime had been present, taking the mean of two results, which did not differ more than 0.03 from each other.

The residual fluid, after it had experienced the full effect of both these re-agents, was evaporated to dryness, and redissolved in an acid; but after carbonate of ammonia had ceased to throw down lime, no indications of magnesia appeared on the addition of the alkaline phosphate.

It would [illegible] that when carbonate of ammonia is added to a [illegible] f two salts, the one containing lime, the other [illegible] proportion of the latter earth is thrown down with the former; so that the quantity of precipitate afterwards afforded by phosphate of soda or ammonia, necessarily falls short of that which would otherwise have been obtained.

But we have no proof that the lime which has resisted the action of the carbonate of ammonia, and remains in the liquor, interferes with the operation of the alkaline phosphate on the remaining portion of the magnesia; so that in this respect, the method appears to be as rigorous as can well be devised.

Having said so much on the operation of the alkaline phosphates upon magnesian salts, it might be expected that I should conclude my account by a statement of the composition of phosphate of magnesia, as well as that of the triple phosphate, deduced from my own experiments, as this has in either case been inferred hitherto rather from synthesis than analysis. Having met, however, with difficulties in my first attempts (which, I believe, are acknowledged by all who have been engaged on this genus of salts), I must decline for the present entering upon this part of the subject, only regretting that the same philosopher [*] who, by so ingenious a mode of operating, appears to have at length set at rest the long-agitated question with respect to the true composition of phosphoric acid, did not at the same time extend his analytical researches to that of the salts which it forms [†].

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[*] See Sir H. Davy's paper on Phosphoric Acid, in the *Phil. Trans.* for 1818.

[†] It may be as well to mention the method I pursued in attempting to analyse Phosphate of Magnesia, which was as follows:

Phosphate of Magnesia (as is well known) may be dissolved by the sulphuric, nitric, and muriatic acids, but chemists are divided as to whether any decomposition of the salt takes place or not. Dalton, in the *Manchester Memoirs*, vol. v. (New Series), maintains that this is the case; but the majority of chemists believe that the sulphuric acid decomposes, and that the others merely dissolve it. However this may be, to effect a solution of this salt in one or other of the acids appears the best preliminary to its analysis. I therefore attempted to precipitate the magnesia from its solution in acids by various re-agents.

The fixed alkalies do not appear, however, to succeed, and ammonia precipitates anew the triple phosphate. By adding a solution of nitrate of lead to one containing 10 gr. of phosphate of magnesia dissolved in nitric acid, I obtained, after some time, very regular tetraedral crystals of a salt, which, when its water of crystallization was driven off, was found to weigh 20.2 gr. On examination, they were found to correspond in chemical characters, as well as in crystalline form, In conclusion, then, I may observe, that the method we have been considering, answers most beautifully the purpose of ascertaining the presence of minute quantities of magnesia, and that it may be also employed as a ready though not a perfectly accurate method of determining the proportion in which this base occurs, even when lime is present; but that if we wish likewise to ascertain the amount of the latter earth, we shall find it necessary, before we add the carbonate of ammonia, to throw down the lime by some other means. Unless, therefore, we can discover some precipitant which will do this completely, without affecting the magnesia *, the formula first mentioned seems of the two the most precise.

III. M. Longchamp (whose paper in the 12th volume of the Annales de Chimie I have already alluded to) proposes to employ ammonia in the state of a subcarbonate, for the purpose of separating the two earths, conceiving that we shall in this manner be enabled to throw down the lime before the magnesia begins to be separated. He asserts, that if the solution be filtered within two hours after the addition of the subcarbonate of am-

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with the nitro-phosphate of lead, which Berzelius mentions having obtained by a similar process, and of which 20.2 grains would be composed, according to his estimate, of 13.47 phosphate of lead, and 6.73 nitrate of lead. Now, 13.47 grains of phosphate of lead contain only 3.36 of acid, so that it is evident that all the phosphoric acid was not precipitated in this manner, for 10 grains of phosphate of magnesia contain abcut 6 gr. of acid. Time has not allowed me to prosecute the subject further.

* This part of my paper was completed, when I was favoured by a letter from Mr H. Warburton, Vice-President of the Geological Society, in which he informs me, that Dr Wollaston considers it advisable, in the first instance, to precipitate the lime by oxalate of ammonia, taking care not to leave an excess of the oxalate, and not to have the solution of the lime too dilute after the oxalate of ammonia is separated. Then to the clear liquor he adds bicarbonate of ammonia, and, lastly, phosphate of soda. "The smallest quantity of magnesia will thus be rendered visible, if the side of a watch-glass containing some of the liquid be touched below the surface of the fluid with a glass-rod." I have had no direct experience of this last method, but can readily believe it to be an improvement upon the one usually employed by me. There could be little difficulty, however, in separating the two earths, if oxalate of [illegible] could be employed, without acting upon magnesia, or being influenced by [illegible]. This, however, I shall endeavour to shew is often the case.

monia, the lime alone will have precipitated; but that after standing a longer time, successive portions of magnesia fall down, until the whole is deposited.

Supposing this statement correct, I should view with some suspicion a mode of analysis, which was dependent for it success upon the nicety with which we timed the succession of our chemical manipulations, and should conceive that the proportion which the precipitant might bear to the substances acted on (to say nothing of other contingencies), might alter materially the length of time which was to intervene before the deposition of the magnesia.

The experiments, too, that are detailed above, shew that the effect of this re-agent is considerably influenced by the proportion which the two earths bear to each other, and even lead to the conclusion, that solutions of the sulphates of lime and magnesia might be mixed in such a proportion with this re-agent, as should render both altogether proof against its influence.

In order, however, to ascertain this point more fully, I dissolved equal parts of muriate of lime and muriate of magnesia in distilled water, and, after adding subcarbonate of ammonia, carefully separated within an hour the precipitant which had fallen down.

On examining it, I certainly found that lime was the principal ingredient, but very distinct traces of magnesia were afforded by the application of phosphate of soda.

On the other hand, a precipitate which afterwards fell, consisted chiefly of magnesia, with some admixture of lime.

IV. I cannot, therefore, recommend this last method to the adoption of chemists, but shall proceed to consider another formula for separating the two earths, namely, that of first getting rid of the lime by oxalate of ammonia, and afterwards precipitating the magnesia by the alkalies, or the phosphoric acid with excess of ammonia.

Confining ourselves to the mode of throwing down the lime, we may observe, that one objection to it has been already suggested, by an experiment which I have detailed in a former part of this paper, where the presence of a large proportion of magnesian salt appeared to diminish the precipitate obtained from a [illegible] of sulphate of lime, through the agency of the [illegible].

To confirm this observation, and likewise to ascertain whether the precipitate of the lime could be altogether prevented by the influence of a predominating portion of the magnesian sulphate, I tried the effects of the test upon mixtures of the two salts, in the following proportions, noting the effects at the time of the addition, and at an interval of one hour, and twenty-four hours afterwards.

[illegible] ammonia added to	*Proportion of the Ingredients, being.*			*Rendered the Liquor*		
	Water,	Sulph. Magn.	Sulph. Lime.	*at the time*	*1 hour afterwards*	*24 hours afterwards*
1. A saturated solution of sulph. of lime.	480		1	very turbid.	very turbid.	very turbid.
2. Saturated solution of sulph. of lime, Solution of sulph. of magnesia 25 gr. to the 1 oz. } equal parts,	960	25	1	very slightly turbid.	very turbid.	very turbid.
3. Sol. of sulph. lime, 1 part. Distilled water, 2 parts,	1440		1	slightly turbid.	very turbid.	very turbid.
4. Sol. of sulph. lime, 1 part Sol. of sulp. magnesia 25 gr. to the 1 oz. 2 parts,	1440	50	1	caused no effect.	less turbid than No. 3.	very turbid.
5. Sol. of sulph. lime, Sol. of sulph. magnesia, containing 50 gr. to the 1 oz. } equal parts.	960	50	1	very slight effect; less than in No. 2.	slightly turbid.	very turbid.
6. Sol. of sulph. lime, 1 Part Sol. of sulp. magnesia 50 gr. to the 1 oz. 2 parts,	1440	100	1	caused no effect.	caused no effect.	slightly turbid.
7. Sol. of sulph. lime, Sol. of sulph. magnesia, 100 grains to the 1 oz. } equal parts.	960	100	1	caused no effect.	slightly effect.	slightly turbid.

It will be seen from the above experiments, that the inertness of the oxalic test cannot be wholly attributed to the mere *dilution* of the sulphate; for in experiment 3. the salt was diffused through the same quantity of water as in No. 4. and 6., and yet the effects were very different. Nor could it be owing to any strange affinity between the oxalic acid and the magnesia, for a reference to th[illegible]mon Tables of Affinity will shew us, that lime decompo[illegible]late of magnesia; and accordingly, if only a few dr[illegible]lution of an oxalate be added to one of muriate, or sulphate of magnesia, so that we may be sure, from the proportion they bear to each other, that the re-agent is not in excess; and if a single drop of this mixed liquor be added to water containing lime in any of its states of combination, the precipitation which ensues will convince us of the correctness of the Tables in this instance.

Although, in the experiments above detailed, I confined myself to solutions of the two sulphates, I have since found that muriate of lime is likewise rendered much less sensible to the action of the same test by the presence of a predominating proportion of muriate of magnesia *; so that we ought always to be cautious in inferring the absence of lime, from the circumstance of no change occurring upon the addition of an oxalate, when the solution is found to contain any considerable quantity of magnesia.

It appears, however, that where the proportions of the two earths present are more nearly balanced, the use of the oxalic test is open to an objection of quite the opposite kind, the presence of magnesia serving in this case to increase instead of diminishing the quantity of precipitate obtained. On this subject, however, I shall bring forward nothing of my own, as my friend Professor Kidd of this University, has been kind enough to put into my hands a statement of some experiments on this subject, instituted by himself many years ago, and therefore without any reference to the present inquiry. The following is an outline of these experiments.

July 14. 1812.—Dr Kidd took 50 grains of the calc-sinter, deposited from the waters at Matlock, and dissolved it in nitric acid. Oxalate of ammonia being then added, until its action ceased, a precipitate was collected, which, when carefully dried at a temperature scarcely exceeding 80°, yielded 71 grains.

2dly, He treated 20 grains of pure carbonate of lime, procured from lime-water, in a manner precisely similar, and obtained 30 grains of oxalate of lime. 50 grains of the same would therefore have yielded 75 grains of the oxalate.

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* This fact, I find, is noticed by Mr Phillips in his paper in the Journal of Science, so often alluded to.

3dly, He took 30 grains of the Matlock calc-sinter, and added to it 20 grains of carbonate of magnesia, both of which being dissolved in nitric acid, and treated with oxalate of ammonia, gave a precipitate which amounted to 51 gr.

Now, as 30 : 51 : : 50 : 85.

Wishing to confirm these results, Dr Kidd next took 50 grains of calcareous-spar, and added to it 33.33 gr. of carbonate of magnesia, both of which were dissolved in nitric acid, and yielded with oxalate of ammonia 86.75.

4thly, He dissolved 30 grains of the same calc-spar with 20 grains of carbonate of magnesia, and obtained by the same re-agent a precipitate which weighed 51 gr.

Now, as 30 : 51 : : 50 : 85.

I shall present the results indicated by these experiments in a tabular form, supposing the quantity of oxalate of lime formed from the pure carbonate, where no magnesia was present, to have been 100 grains, and reducing the others to the same ratio.

		Oxalate of Lime.
Carbonate of Lime from lime-water, gave,		100 gr.
——— — —— from the Matlock calc-sinter,		94.75
Carbonate of Lime from calc-spar,	3 parts,	114.49
Carbonate of Magnesia,	2 parts,	
Carbonate of Lime from the Matlock calc-sinter,	3 parts,	113.33
Carbonate of Magnesia,	2 parts,	

The remarkable coincidence between these results, which, with the other particulars, I have been permitted to extract from the manuscript notes preserved by Dr Kidd, of a miscellaneous set of experiments on various limestones, seems to establish the general law, that when a large proportion of magnesia is present, in conjunction with lime, the precipitate thrown down by oxalate of ammonia exceeds by more than $\frac{1}{10}$th that which could be obtained by the same reagent from similar proportions of carbonate of lime, without any such admixture.

Were we sure, indeed, that the increase was always in the ratio indicated by Dr Kidd's experiments, our opinion as to the accuracy [illegible] might be less affected; but as the experiments be [illegible] l to quite the opposite conclusion to this, it woul [illegible] ist to the oxalic test for the complete separation of the two earths, or for determining their relative proportions.

Other methods for effecting the separation of the two earths have been suggested; but the report made by Dr Thomson, and other able chemists, on their merits, is not very encouraging.

One of these is to precipitate the lime by means of bicarbonate of potass, which, it was presumed, would retain the whole of the magnesia in solution; but this has been shewn by Bucholz not to be the case,—a portion of the lime being retained, whilst a part of the magnesia is precipitated.

Another, recommended by Professor Dobereiner of Jena, is to dissolve the magnesia, reduced to the state of a carbonate, in a solution of muriate of ammonia, thus separating it from the carbonate of lime, on which he represents this salt as having no action.

I have shewn, however, in a preceding part of my paper, that this statement is not strictly correct, even when the carbonate of lime is unmixed, and Professor Pfaff of Kiel has proved, that the co-operating affinity of the magnesia causes a portion of the lime to be taken up, by the ammoniacal salt, whenever the two earths exist together.

On these methods I have nothing further of my own to offer, and only notice them in order to complete this sketch; but there are other schemes of analysis that have occurred to me, on which I have a few observations to make, before I conclude this part of the subject.

One of these is founded on the readiness with which magnesia parts with its acid, under an increased temperature, an instance of which we have already met with, in the partial decomposition of sulphate of magnesia, upon exposure to a red heat. The muriate being still more readily affected by the same agent, I conceived that it might be possible to separate it from muriate of lime by this method, as the latter, when submitted *per se* to a red heat for some hours, appeared to lose none of its acid. I found, however, that even after several hours exposure to this temperature, the muriate of magnesia was but imperfectly decomposed, for the insoluble base which remained, when dissolved in nitric acid, indicated by its colour the presence of muriatic acid, and gave a precipitate with nitrate of silver. This precipitate was found to weigh 0.27, equivalent to 0.05 of muriatic acid; the insoluble residuum amounting to 5.8 grains of hydrate of magnesia. The 4.2 grains which were driven off, consisted partly of acid and partly of water; but it is difficult to determine the relative proportions of each in this manner, because muriate of magnesia cannot well be freed from water, without decomposing a part of its acid. I found, too, that when muriate of magnesia and muriate of lime were heated together, the loss exceeded that which could have resulted from the same quantity of muriate of magnesia alone, and that this must be attributed to the decomposition of the calcareous salt, was evinced by the effect of oxalate of ammonia upon the precipitate, when dissolved in an acid. From some experiments which I made, it seemed probable that the increase amounted to about 10 *per cent.*; but the difficulty of procuring muriate of magnesia perfectly dry, without diminishing its solubility, by driving off the acid, induced me to vary the experiment in the following manner: I took 5 grains of carbonate of magnesia, dissolved it in muriatic acid, and having driven off the excess of acid, exposed the salt to a red heat for half-an-hour. The residuum being then treated with water, in order to separate the soluble from the insoluble portion, that which remained on the filter being dried was found to weigh 1.42, and what had been taken up by the water, of which nearly half a pint was employed, 0.24 gr. Both these portions were found, however, to be submuriates; for nitrate of silver gave a precipitate with both of them. In order to learn whether muriate of lime would undergo decomposition, when muriate of magnesia was present, I took 5 grains of a pure carbonate of lime, and having dissolved it in muriatic acid, obtained a salt, which, when dried by exposure to a red heat, weighed 5.2. I then dissolved 5 grains of the very same carbonate of lime in muriatic acid, with an equal quantity of the same carbonate of magnesia as that employed in the preceding experiment, and having driven off the excess of acid, exposed the mixed salt to the same degree of heat as that employed pre[illegible]. Water being added to the residuum, left an insolu[illegible] amounting not to 1.42, but to 1.73, and took up [illegible] of 5.2 grains, which the preceding experiment p[illegible] been the result, if the same portion of muriat[illegible]en employed alone.

As a further proof that a portion of lime was contained in the insoluble residuum, I dissolved it in nitric acid, and found that oxalate of ammonia gave a manifest precipitate.

From the above facts, I think we may conclude, that the existence of magnesia tends to facilitate the decomposition of muriate of lime, although it seems much less easy to determine in this than in the foregoing instances, in what manner the result can be influenced by its presence. I know not, indeed, whether my readers may not be more disposed to substitute for this explanation, the idea of a portion of the calcareous salt being preserved from the action of the water, by the affinity it bears to the magnesia; but be the explanation what it may, the circumstance I have alluded to must be considered a serious additional obstacle to this method of analysis.

Lime-water appears likewise to throw down magnesia; but it is so difficult to get rid of the carbonic acid which is extricated during the solution of the limestone in an acid, and is absorbed by the water, that we can seldom be sure that this may not be the cause of the cloudiness resulting from its addition *. Besides this objection to the use of lime-water, there is that of its throwing down alumine, which so commonly enters into the composition of magnesian limestones; nor does it appear to separate the whole of the magnesia that may be present.

Fluate of ammonia also appears to me to furnish a method of distinguishing the two earths, although my experiments have not enabled me to determine, whether this test can be employed with advantage as a means of separating them. The above salt, indeed, not only has the property long ago attributed to it by Scheele, of forming an insoluble salt with lime, but also produces the same effect with magnesia; there being, however, this difference, that, with the former, the precipitate is from the first insoluble in acids, while, with the latter, it only becomes so by slow degrees.

Hence, if an acid, which exerts a weaker affinity for the two bases than the fluoric does, such, for instance, as distilled vinegar, has been previously mixed with a solution suspected to con-

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* I believe that carbonic acid is always present in salts obtained from carbonates, unless the solution has been evaporated to dryness, and redissolved in water recently distilled. It is not easy to remove it by simple boiling.

tain the two earths, I find, that the addition of fluate of ammonia will cause a milkiness if lime be present; but no effect with magnesia.

Should a cloud, therefore, be produced in the acid solution, when fluate of ammonia is dropped in, we must continue to add the salt so long as any effect continues, then filter the liquor, and apply a heat sufficient to expel the acetic acid, when, if any magnesia be present, a fresh milkiness may be expected to arise.

From the foregoing statement, I think we are warranted in concluding, that of the methods practised for separating lime from magnesia, that founded on the principle of converting the two earths into sulphates, is probably the most unexceptionable; and this, accordingly, is the one I have generally pursued in the analysis of a magnesian limestone; although, to ascertain the presence of magnesia merely, I have found Dr Wollaston's by far the easiest and most expeditious.

The plan, perhaps, which answers best, is the following.

Dissolve a given portion of the earth in nitric acid, thus separating the silex, and most of the peroxide of iron, from the lime, magnesia, alumine, protoxide of iron, &c. Evaporate the solution, and drive off the nitric acid by heat, thus converting the iron into a peroxide. Weigh the residuum after calcination, and treat it with distilled vinegar, which will only take up the lime and magnesia.

Subtract the weight of the undissolved portion from that of the residuum after calcination, thus ascertaining the joint weight of the lime and magnesia; then decompose the acetates by sulphate of ammonia; evaporate the solution nearly to dryness, and separate the sulphate of lime from the sulphate of magnesia, by repeatedly washing it with water, already saturated with sulphate of lime, as Mr Phillips has recommended. Expose the sulphate of lime to a red heat, and ascertain its weight, from which the amount of the lime originally present may readily be deduced. We may then infer the weight of the magnesia from the difference between the amount of the lime, and that of the whole which the vinegar was found to have dissolved; or if we wish to verify [illegible] it may be done by precipitating the magnesia by [illegible] better, by converting it into a triple phosphate [illegible] of sulphate of lime present in the water, used for washing off the sulphate of magnesia, may be estimated and allowed for.

If this process be considered too tedious, we may content ourselves with dissolving the two earths in muriatic acid, decomposing the solution by sulphate of ammonia; and after suffering the sulphate of lime to subside, decanting off the supernatant liquor, and throwing down the magnesia from the latter by carbonate of ammonium and phosphate of soda. In this way an approximation to the real composition of the earth may be readily obtained.

It now only remains for me to speak of the re-agents, which, where no other base is present, may be employed for the purpose of precipitating magnesia, in the state either of a pure earth or a carbonate.

We are commonly told, in Elementary Works on Chemistry*, that the alkalies and alkaline carbonates, throw down all earthy and metallic salts; the only exception made, being as to the carbonate of ammonia, which, it is well known, has no such effect upon magnesia.

Accordingly, we are directed in our analyses, to throw down the lime by means of carbonate of ammonia, and the magnesia, by adding subsequently the pure alkali; whereas the fact is, that neither process effectually answers the purpose designed.

The tendency, indeed, which magnesia has to form triple salts, or, in other words, its affinity for the alkalies, interferes considerably with the result in all these cases; for I have reason to believe, that, owing to this circumstance, a part of the magnesia is often retained in the solution with the alkaline salt, and, perhaps, a trace of the alkali carried down along with the earth.

I believe it to be in part owing to this circumstance, that I have failed in obtaining the quantity of precipitate from a given weight of sulphate of magnesia, which I had expected from calculation, when the reagent employed was either ammonia, soda, or its subcarbonate †.

Whenever these substances were made use of, I have found, that after they had been boiled for hours with magnesian solution, although the alkali continued at the end of the experiment to be in excess, the presence of a larger portion of magnesia was detected in the filtered liquor, by adding carbonate of ammonia with a phosphoric salt, than could be accounted for, from the solubility of magnesia and its carbonate in the water.

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* See Henry's Elements, &c. &c.

† When ammonia was employed, the quantity of precipitate varied considerably, as Berzelius has also found. (See his paper on Compounds, which depend on weak affinities. *Edin. Phil. Jour.* vol. i. p. 253.) With soda, the quantity obtain-

Nevertheless, I am ready to allow, that, by adding a considerable excess of the reagent, soda, and still more effectually potass, together with their respective subcarbonates, may succeed in throwing down the whole of the magnesia present; for the smallest quantity of the latter dissolved in water, or united with an acid, gives a flocculent precipitate in solutions of these alkalies. It is right, however, that we should be aware, that the existence of a slight excess of the reagent in the solution, is no sufficient evidence that the whole of the earth has been separated.

There is another objection to the use of these reagents, where extreme exactness is required, namely, that the precipitate caused by them, often appears to retain a trace of the acid with which it was originally united.

I have the authority of Berzelius for stating this fact, with regard to the precipitate afforded by ammonia; and when the magnesia was thrown down from its sulphate by pure soda, I have found, that, on being redissolved in muriatic acid, it continued to give decided indications of the presence of sulphuric acid, even when the distilled water, with which it had been washed, ceased to produce the slightest alteration in a solution of muriate of barytes.

These experiments, however, must be repeated, before I can feel myself warranted in arriving at any fixed conclusion, on a point which involves so important a question of chemical theory; but it may be useful to mention the results I have obtained, however insufficient they may be to decide the question, if it

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ed from 50 gr. of crystalline sulph. magnesia, containing 8.0 of base, was 6.9 taking the mean of [illegible] periments; by subcarbonate of soda, 7.2. The precipitates by potash [illegible] ates seemed to be somewhat larger; but the sparing solubility [illegible], renders the employment of the latter reagents to throw do[illegible] s sulphate somewhat inconvenient.

were only as cautions to those who may employ these reagents in precipitating magnesia from its solutions.

The objections that have been raised against these, do not apply to Dr Wollaston's plan of converting the earth into a triple phosphate with ammonia, as the peculiar tendency to form triple salts, which renders the precipitation of magnesia from its solutions in other cases imperfect, is here beautifully employed to serve the very purpose of separating it from other earths. This latter circumstance, as well as the insolubility of the precipitate thus obtained*, gives this method of analysis a decided superiority over that by the pure alkalies or their subcarbonates.

And now having brought to a conclusion my remarks on the methods of separating magnesia from lime, it only remains for me to apologize for the length to which this part of my paper has extended. Had I, indeed, consulted my own ease and that of my readers, I should have, undoubtedly, contented myself with merely stating the results to which my enquiries had led, without entering into the detail of the experiments themselves;— a mode which, whilst it enabled me to compress my materials into a narrower compass, would have laid me much less open to criticism, than the plan I have chosen to adopt. I felt, however, that (particularly in the first chemical paper I ever presented to the public) I had no right to expect that other persons should take for granted my conclusions, unless satisfied as to the road by which I had arrived at them, and enabled fully to judge for themselves, how far the several steps of this inquiry have been correct, and in what respects they may have diverged into error.

ART. XIX.—*Account of The Jeffersonite, a new Mineral discovered at the Frankline Iron-Works, near Sparta, New-Jersey. Described and analysed by* WILLIAM H. KEATING, Member of the Wernerian Society.

ABOUT six miles to the north-east of the town of Sparta, in Sussex county, New-Jersey, are to be seen the remains of the old Franklin furnace. This furnace, situate on one of the most beautiful and eligible spots for the working of iron, offers a striking example of the failures which attend all works, which are not conducted with a sufficient degree of attention to scientific acquirements. Placed in the centre of an extensive forest, with an abundant supply of water, surrounded by numerous and inexhaustible beds of ore, at a convenient distance from two good markets, the Franklin works must have appeared to their first owners calculated to become of the highest importance; and such most undoubtedly would have been the result, but for one difficulty which intervened, arrested the operation, and, after many fruitless attempts, caused the total abandonment of the works. This difficulty was, it is true, of vital importance. It arose from ignorance as to the nature of the ore intended to be worked, and of the minerals which accompany it. Having long attempted to work by the common process, an ore which was of a distinct nature, and which, consequently, required a distinct mode of treatment, they at last threw up in disgust an undertaking which very little science would have made highly productive. To the late Dr Bruce, we are indebted for the first light thrown upon this interesting section of our country, and to him the honour of the discovery of the red zinc ore is due. This was, undoubtedly, the first step toward the advancement of that section of the country. The next, and a more important one, was the determination of the real nature of the substance, which had hitherto been considered as a common iron ore, and which is now known under the name of Franklinite, as a combination of the oxides of iron, zinc, and manganese. This discovery was made in the laboratory of the Royal School of Mines in Paris, in the spring of 1819, and has been published by Professor Berthier, in the fourth volume of the "Annales des Mines," 1819.

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* Both magnesia and its carbonate are soluble in water, as is proved by Dr Fyfe in the *Edin. Jour.* vol. vi.

Having, in the month of August last, visited this spot with my friend Lardner Vanuxem, Esq. of the South Carolina College, our attention was directed with peculiar pleasure to a bed of ore, which offered a number of new and interesting varieties of minerals, and which we think bids fair to become as celebrated in mineralogy as the localities of Uto or Arendal.

It is not my object at present to enter into an enumeration of the minerals which occur there. I shall merely state, that the minerals which we collected, include, besides the oxidule of Iron, Franklinite crystallized in regular octahedrons with truncated edges, Garnets of various kinds, and, among others, a black emarginated dodecahedral Garnet, analogous to the Melanite of Monte Somma, Chondrodite, the same variety as exists at Sparta, and also a new variety of the same substance, besides many other interesting minerals.

The present communication is intended to make known a mineral, which an attentive examination, made by Mr Vanuxem and myself, has induced us to consider as a new species. Our observations upon this mineral, made separately, and at a distance, have led us to the same conclusion; and the analysis which I undertook at his request, has fully confirmed the conclusions drawn from its mineralogical characters. The following description includes, besides my observations, those with which Mr Vanuxem has favoured me. For the results of the chemical analysis I alone stand answerable.

This mineral has hitherto been found in lamellar masses, the largest of which does not exceed a pigeon's egg, embedded in Franklinite and Garnet.

It presents three distinct cleavages, two of which are considerably easier than the third. These cleavages lead us for a primitive form to a rhomboidal prism, with a base slightly inclined. The angles of the prism are 106° and 74°, those of the inclination of the base are 94° 45′ and 85° 15′. There is another face, which makes with the vertical face of the prism, angles of 110° and 70°. I have likewise seen, in one instance, cleavages parallel to a rhomboidal prism of 116° and 64°. I have also obtained cleavages under an angle of about 99° 45′ and 80° 15′. I have not been able to trace the connection between these and the former, but I am inclined to think, that they result from the combination of the two prisms just mentioned. I had hoped, as some of the cleavages have a tolerable degree of lustre, to have been enabled to determine the angles by the reflecting goniometer, but all my attempts to that effect have proved unsuccessful. I have not been able to obtain a reflection from any one face.

The hardness of this mineral is intermediate between that of fluor-spar and apatite. It is very readily scratched by pyroxene, (Malacolite).

Its specific gravity varies from 3.51 to 3.55. I have in one instance obtained it as high as 3.64, but I suspect the mineral to have been mixed with Franklinite.

Its colour is dark olive-green, passing into brown.

It is slightly translucent upon the edges.

Its lustre is slight, but semi-metallic upon the faces of cleavage; in the transverse fracture it is resinous.

The fracture is lamellar, when in the direction of cleavage, otherwise it is uneven.

When scratched with a knife, the streak is greyish.

The colour of the powder is a light-green.

Before the blowpipe it melts readily into a dark-coloured globule.

It displays no electric signs, either naturally or by heat or friction.

It is not magnetic, either in the common way, or by the ingenious method of double magnetism, which we owe to Abbé Haüy.

The acids do not act upon it when cold. When digested a long time with boiling nitro-muriatic acid about $\frac{10}{100}$ is dissolved. The residue is of a lighter colour.

Its chemical composition was ascertained by two analyses, the results of which were strikingly similar, and were as follows:

Silex, - - - -	0.560	containing oxygen 29.14700	29
Lime, - - -	0.151	4.24159	4
Protoxide of Manganese,	0.135	2.95790	3
Peroxide of Iron, -	0.100	3.06600	3
Oxide of Zinc, - -	0.010		
Alumine, - - -	0.020		
Loss by calcination, -	0.010		
	0.986		
Loss, - -	0.014		
	.000		

By assuming [illegible] al formula $4\,C\,S^5 + 3\,mg\,S^3$

$+ 2FS^3$, which is very nearly that indicated by the results of the analysis, we have for the chemical formula,

$$4\ddot{C}a\ddot{S}i^2 + 3\ddot{M}n\ddot{S}i^2 + 2\overset{\cdots}{F}e\ddot{S}i^3,$$ which gives us

Silex, - - -	20 atoms, containing	60 atoms oxygen.
Lime, - - -	4	8
Protoxide of Manganese,	3	6
Peroxide of Iron, -	2	6

Which proportions are so near those of the results of the analysis, that we may adopt them without hesitation. From this formula, we obtain the true composition of this mineral to be,

4 at. *Trisilicias Calcicus*, -	7619.60
3 at. *Trisilicias Manganosus*,	6313.23
2 at. *Trisilicias Ferricus*, -	5535.38
1 *atom new mineral*, -	19468.21

Or else,

20 Silex, - - - -	11928.40
4 Lime, - - - -	2848.24
3 Protoxide of Manganese,	2734.71
2 Peroxide of Iron, - -	1956.82
	19468.21

Which reduced to 10000 parts, gives

Silex, - - -	0.6125 containing Oxygen	3.0808750	30
Lime, - - -	0.1463	.4109567	4
Protoxide of Manganese,	0.1404	.3080376	3
Peroxide of Iron, - -	0.1005	.3081330	3
	0.9997		

This mineral will therefore present, in the new and expressive language of Mr Berzelius,

$4CS^3 + 3mgS^3 + 2FS^3$ for its mineralogical, and

$4\ddot{C}a\ddot{S}i^2 + 3\ddot{M}n\ddot{S}i^2 + 2\overset{\cdots}{F}e\ddot{S}i^3$ for its chemical formula.

According to Professor Mohs' new and elegant mode of classifying and describing minerals, this would form a new species, in his genus Augite Spar, and come immediately after the Pyramido-prismatic Augite Spar (Pyroxene, Haüy). On account of its many cleavages, I would propose to give it, as a specific name, the epithet of *Polystome*. It would therefore be thus designated:

CLASS I.—ORDER SPAR.
Genus, AUGITE SPAR.
Species, *Polystome Augite Spar.*

Prismatic. P, unknown. Cleavage $P+\infty=106^\circ$. $H=4.5$. $G=3.51$—3.60.

But until Mr Mohs' system be more generally known and approved of, it may be proper to give this mineral a name unconnected with his arrangement. Accordingly, Mr Vanuxem has proposed to dedicate this mineral to Mr Jefferson; I have readily assented to this proposal, and we now offer this mineral to the public under the name of *The Jeffersonite.*

This mineral has hitherto been found in too small a quantity, to offer any utility in the arts. Should it, however, be found in sufficient abundance, it would become valuable as a flux for the iron-works in the vicinity. The absence of magnesia, and the abundance of manganese, seem to make it very valuable for this object.

The Jeffersonite presents some points of resemblance with the Pyroxene of Haüy, but still it can be well distinguished from it. Its cleavages are essentially different from those of the Pyroxene, but appear to approach some of the faces of crystals of substances which have been united to this species: for instance, the angles in the Diopside (Mussite and Alalite), Fassaïte, and in the *Pyroxene analogique,* come very near some of the angles of cleavage obtained in The Jeffersonite. I at first indulged the idea, that these cleavages might be considered as cleavages parallel to the faces of secondary crystals of Pyroxene, but upon reflection I am fully convinced that this is not the case; for the angles which we have measured, cannot be deduced from the others by a strict mathematical calculation, and though they may approximate, they are not the same. Besides, no analogy can warrant us in admitting, that the regular cleavages of one substance can disappear entirely, and be replaced by cleavages parallel to secondary crystals. On the contrary, wherever minerals have been found presenting different orders of cleavage, the first, or those [illegible] to the primitive form, were always predominant. Thus [illegible] of lime, it is not uncommon to meet the cleavage [illegible] *uiaxe*, but I believe in every instance the [illegible] minant. In a rarer and more interesting instance, that of fluor-spar, Professor Mohs has described, and I have seen in his possession in Freyberg, specimens of the Saxon fluor, which cleaved in the direction of the cube and the dodecahedron, but the octahedral cleavage was very distinct. Before we change our opinion on this point, we must change all our ideas of cleavage, and of its high importance in the determination of minerals.

In the hardness there is also a remarkable difference, the Pyroxene being decidedly harder. The specific gravity is likewise different: the highest specific gravity of Pyroxene recorded by Haüy, is that of a large crystal from Vesuvius, which gave 3.3578. The highest specific gravity indicated by Mohs is 3.5, while that of The Jeffersonite has, in every instance which I have seen, exceeded this limit.

The chemical analysis offers another important difference, in the absence of magnesia, which appears to be essential to Pyroxene.

For these and other reasons, I conceive that there can be no doubt as to the necessity of considering this mineral as a distinct species. I am inclined to believe, that a closer study of the Diopside and Fassaïte, and of the *Pyroxene analogique*, might lead to their separation from the Pyroxene and union with The Jeffersonite. This is a subject which appears to me fraught with interest, but upon which I am not able to offer any thing but conjectures, as my specimens of these minerals are not as good as would be necessary, to enable me to decide this point. I shall close these remarks merely by observing, that a similar opinion is, I believe, entertained by Mr Vanuxem.

ART. XX.—*Description of a New Reflecting Telescope.* By DAVID BREWSTER, LL. D. F. R. S. L. & Sec. R. S. Edin.

THE only improvements on the Reflecting Telescope which have been proposed during the last century, are those of Lemaire and M. Burckhardt. The first of these authors suggested the idea of employing what is called the *Front View* in Newtonian telescopes of a large size; and this method was found of great value in the magnificent instruments of Sir William Herschel. In this case, the small speculum was laid aside; and the observer, placed with his back to the object, looked through an eye-piece placed on one side of the mouth of the tube, while the rays issuing from the object entered at the other side. By this means, a second reflexion was avoided; and though a slight distortion in the image must have resulted from the obliquity of the first reflexion, yet the inclination of the incident and the reflected rays was too small to produce any perceptible error.

In order to shorten the reflecting telescope, M. Burckhardt of the Academy of Sciences proposed to substitute a plane speculum in place of the small concave speculum of the Gregorian telescope. By giving the plane speculum *half* the diameter of the large one, and placing it *half-way* between the large speculum and its principal focus, it intercepted the cones of rays at the middle of their axis, and reflected them to foci within the perforation of the great speculum *. In extending this principle to the Newtonian telescope, M. Burckhardt proposed that the plane speculum (of the same size, and similarly placed, as in the preceding construction), should be inclined to the reflected rays, so as to throw the image above the great speculum, where it is viewed by the eye-glass.

The disadvantages of these constructions are very obvious, and are by no means compensated by the trivial advantage of shortening the telescope. The difficulty of giving a good figure to a plane speculum one-half the diameter of the large one,—the loss of light by a perpendicular reflexion,—and the circumstance of the plane speculum intercepting such a large portion of the incident rays, will prevent this construction from being carried into effect †.

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* This construction was proposed by James Gregory himself so early as 1672. See David Gregory's *Elements of Catoptrics and Dioptrics*, 2d edit. p. 261, 262.

† Mr Paterson had previously constructed a Newtonian telescope upon the very same principle as that of M. Burckhardt; but with this difference, that the plane speculum was placed at one side, so as not to intercept any of the incident rays, and consequently th[illegible] speculum inclined to the incident rays. This, however, cannot be consider[illegible]ment; for, though it removes the objection arising from the loss o[illegible] the much greater evil of an oblique reflexion, and consequently [illegible]n telescopes of 5 feet focal length, and an

We believe it will be admitted by every person who has had much experience in the use of reflecting telescopes, that the Newtonian form is decidedly the best. Its construction is so simple, and it is so little liable to derangement from accidental causes, that, for popular use, on a small scale, it is superior to the Gregorian one, while, for instruments of great size, it is the only form that is practicable. But even when we consider it in a scientific point of view, it has the advantage of the Gregorian form. It is more easy to give a perfect figure to an uniform circular piece of metal than to a perforated disc;—the spherical aberration is less, in so far as it is not increased by a second spherical mirror;—and the quantity of light reflected from the oblique small speculum is decidedly greater than when it is reflected at a vertical incidence.

If we could dispense with the use of the small specula in telescopes of moderate length, by inclining the great speculum, and using an oblique, and consequently a distorted reflexion, as proposed first by Le Maire, we should consider the Newtonian telescope as perfect; and on a large scale, or when the instrument exceeds twenty feet, it has undoubtedly this character, as nothing can be more simple than to magnify by a single eye-glass the image formed by a single speculum.

As the *front view* is quite impracticable, and indeed has never been attempted in instruments of a small size, it becomes of great consequence, in a practical point of view, to remove as much as possible the evils which arise from the use of a small speculum. These evils may be thus enumerated.

1. If we suppose the light reflected by the large speculum, or the light incident upon the small one, to amount to 10,000 rays, then the light reflected by a well polished plane speculum will not exceed two-thirds of this, or 6666 rays, at a vertical incidence, and probably not above 6900 at an incidence of 45° [*]; consequently 3200 rays out of 10,000 are lost by the use of the plane speculum.

2. Besides this great loss of light, we have to encounter all the errors of a second reflexion, arising from imperfections of

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aperture of 7½ inches, the inclination of the incident and reflected measures is about *eleven degrees.*

[*] The law of variation is yet undetermined.

surface, and also from imperfections of figure, as it is universally admitted that a surface of glass is much superior to a metallic surface; and Newton has himself remarked, "that every irregularity in a reflecting superficies, makes the rays stray *five or six times* more out of their due course than the like irregularities in a refracting one *."

The construction by which I propose to remedy these disadvantages, is shewn in Plate V. Fig. 1. where AB is the speculum reflecting the parallel rays RA, RB to a focus at F. The cone of rays AFB is intercepted by an *Achromatic Prism* GH, which refracts them to foci at *f*, where a distinct image is formed in the anterior focus of the eye-glass E, by which it is magnified. The compound prism GH being composed of a prism of crown-glass G, and a prism of flint-glass H, united by a cement of a mean refractive power, the loss of light sustained by the pencil in its transmission through the two, will not exceed 600 rays out of the 10,000, as the light transmitted through a lens of glass is, according to Sir William Herschel's experiments, 94850 out of the 10,000 incident rays. Hence the light lost by transmission through the prism is not *one-fifth* part of the light lost by reflexion, and the errors of reflexion arising from defects of surface and of figure are also incomparably less. As the refracting angle of the prism G will require to be larger, in order to produce a given deviation FH*f*, when it is opposed by the refraction of the flint-glass prism H, we may place the correcting prism H nearer the focus *f*, as shewn in Fig. 2. and make it of crown-glass; or it might even be placed at *h*, beyond the focus, and in contact with the lens E. If this should be found advantageous, the prism and the lens might be formed out of one piece of glass, or a single hemisphere of glass might be used, the eye-hole being placed at such a point of the hemispherical surface, as to obtain a variable prism of the required angle united with a plano-convex lens †.

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* The deviation of a ray arising from the imperfections of a reflecting surface is to the deviation arising from the imperfections of a refracting surface as *four to one*, when the refraction is made from glass into air, and as *six to one* when the refraction is made [illegible]s.

† Every plano [illegible] ano-concave lens, may be converted into a lens and a prism c[illegible] the eye-hole from the vertex of the lens to one side.

As Dr Maskelyne found that the Greenwich Newtonian telescope was greatly improved by inclining the speculum, or rather its axis, about 2¼° to the axis of the tube, it follows, that it performed best with oblique pencils in a certain direction. Before fitting up this telescope, therefore, or indeed any Newtonian telescope, it should be ascertained, by turning the speculum in its cell, and giving its axis various degrees of obliquity, whether or not its performance is improved. If this is found to be the case, and that the angle of inclination is, for example, 2¼°, we shall then require a less angle in the prism G, as part of the deviation of the pencil to the side of the tube is already produced by the new position of the axis of the speculum.

It is obvious, that any rays incident on the prism GH, parallel to the tube, will be refracted upon the side of the tube, and will have no effect upon the image; but if they should be thought injurious, we have only to place an opaque screen, of the same size as GH and its arm, somewhere between GH and the mouth of the tube, so as not to interfere with the cone of refracted rays GH *f*.

This form of the reflecting telescope enables us to construct a very simple double image micrometer, the prism H, either when united with G, as in Fig. 1. or when separated as in Fig. 2. being made of a doubly refracting crystal, cut in such a manner as both to correct the colour of G, and produce the two images.

In viewing eclipses of the sun, or the lunar disc, or any other celestial phenomenon, where there is either too much light, or more than is necessary, a telescope may be fitted up to permit more than one person to see through it at the same time, by allowing a portion of the cone of rays to be refracted through another prism to an eye-glass at the opposite side of the tube, or even by having four prisms refracting a fourth part of the cone to each of the four sides of the tube. The same effect might be produced in the common Newtonian telescope, by using a pyramidal small speculum, with the planes meeting at a vertex placed in the centre of the cone of rays, and inclined to one another at angles of 90°, and to the axis of the telescope at angles of 45°. Another person at A might also be made to see the eclipse by means of a parallel plate of glass at *m n*, inclined so as to reflect a portion of the cone of rays to A. In this way, five persons might see through the telescope at the same instant.

Art. XXI.—*On the Physiology of the Root in Plants.* By

John Murray, Esq. F. L. S., M. G. S., & M. W. S., Lecturer on Chemistry. Communicated by the Author.

The fibres of the root have been generally if not universally considered as capillary tubes, destined to supply the plant with nutriment from the soil. But I am not aware of any direct experiments instituted on the subject, and recorded in the annals of science.

Several circumstances concur to shade this received opinion with doubt.

Van Helmont planted a willow in a given weight of earth. It was during five years carefully supplied with distilled water, and, at the termination of this period, it weighed 169 lb. 3 oz.: while the earthy mass had sustained only a loss of 3 oz. In an analogous experiment made by Mr Boyle, the earth experienced no perceptible diminution. In the experiments of Duhamel and Bonnet, together with those of Tillet, Hales, and Braconnot, we find confirmation of the same interesting fact.

The Tulip, Persian Iris, Jonquil, Narcissus, Hyacinth, &c. grow in bulb-glasses, supplied with pure water. White Cress and Mustard spring up readily on wetted flannel, and in the moist grooves of the "lithovasa." The Bean, Pea, &c. may be reared in damp cotton. Washed sand will serve others. These facts speak for themselves, and need not be extended.

The "Epidendrum flos aëris," or air plant, with the phenomena presented by the "Ficus Australis," recorded in a former number of the Edinburgh Philosophical Journal, and which I myself saw luxuriant some months afterwards, shew that plants can exist independent of earth.

The "Mysembryanthemum," "Cactus," &c. are little indebted to [illegible] earth with which they are supplied, and I never wi[illegible] actus spinosissimus" in such grandeur and majest[illegible] ing from the surface of an arid rock.

The Do[illegible] toe *may* drink up the blood of the nobler tree, but have no communication with the gross ingredients of the soil.

The delicate vessels of the root are not suited to the absorption of carbonaceous matter from the earth, and still less for terrene and metallic particles, ever found in the ashes of incinerated vegetable matter. It is only to gaseous bodies that such subtle structures can be supposed applicable; and I infer from my experiments that such are *excretory* vessels, not absorbents.

Silica and alumina serve a mechanical part in soils, and lime acts chiefly chemically, in decomposing the salts of iron, and animal and vegetable matter supplied as manure; the gaseous bodies thus liberated escape gradually from the surface soil, and form a healthy atmosphere for the plant, by which it is decomposed and assimilated, and forms the proximate or immediate principles of plants.

Burnt clay and dry chopped (undecomposed) straw, are said to act as manures, but such can afford no nourishment to plants. The relations which earths bear to temperature, and to moisture, in their absorbent and retentive characters, should always be taken into the estimate, and the action of burnt clay may be explained in this way.

While in London, last winter, I made a considerable number of experiments on the hyacinth, &c. growing in bulb-glasses. The bulb, being carefully washed with distilled water, was seated on the glass filled also with distilled water, and the whole covered with a bell-glass. In two or three days the water was highly saturated with carbonic acid gas, and this being precipitated with lime-water, potassa or caustic baryta, afforded a brisk effervescence on the affusion of diluted acid. The immediate *milkiness* which ensued on agitating the fluid with lime-water, was proof enough, though it was well to carry the experiment to its ultimatum. In numerous repetitions I found it uniform, and shewed it to some of my friends.

By using lime-water, much diluted with distilled water, the interior surface and bottom of the bulb glass were encrusted with minute rombs of carbonate of lime, perfectly diaphanous.

From a seedsman in Fleet-street, I got a bulb and bulb-glass; the roots had already shot down fibres into the water four to five inches long, and it was fast advancing into flower. The fibres of this plant, when I received it, were ragged at the tips, and ringent or gaping, and they were also quite transparent. Water had filled these tubes, and gave rise to a beautiful phenomenon; for the descent of the air-bells was thus exhibited, and closed the evidence, if farther proof had been necessary.

This fact will certainly tend to explain some apparent anomalies. In experiments made on plants, in relation to their amelioration of the atmosphere, contaminated by respiration, wherein no beneficial change (or a bad one) was exhibited, it must be evident, that as carbonic acid gas is excreted by the roots, the confined atmosphere might be deteriorated by the gas; when the soil is saturated, arising from the earth in which the vegetable grows. Besides, these experiments may change our views, in relation to the phænomena of agriculture, while it will satisfactorily explain the prompt transit of caustic earths into carbonates; and thus may be a hint valuable even to the *geologist*.

Cress, *mustard*, *the pea*, &c. in my experiments, did not even swell in *ether*, *sulphuret of carbon*, or *alcoholic solution of iodine*. Water perhaps serves chiefly to soften the membrane of the seed, or to dissolve any gummy or rather *albuminous* matter, which may seal up the orifices of the excretory, or other vessels. I find that when the *pea*, *bean*, &c. remain some time in distilled water, and such swell, the fluid becomes *opake*. It is *blackened* by sulphuric acid, and *white flakes* or *threads* appear on being heated with *alcohol*, *muriatic acid*, &c. or with a temperature amounting to 176° Fahrenheit, and that it has all the characters of *albumen*.

Carbonic acid distils copiously from the hilum or scar, particularly noticeable in the pea or bean; and here the air-bells will appear ranged in parallel rows. I threw a bean, when the *testa* or skin was rent, and the *corculum* advancing into the attitude of the *plumula*, into distilled water, heated to 100° Fahrenheit. The carbonic acid gas spun out in a continued streamlet for some time, and it issued from the end of the hilum adjoining the corculum. The hilum is almost always studded over with air-bells of carbonic acid gas.

In the course of my researches, I have met with nothing to disturb the opinion I have uniformly maintained, on the beautiful and beneficent reciprocity which exists between the animal and vegetable kingdom; and rather the more am I convinced that neither of them can say to the other, "I have no need of thee."

The experiments of Priestley, Saussure, Sennebier, and Ingenhousz, are very clear and explicit; and even Sir Humphry Davy has set his seal to the doctrine. But in all these, and others of more modern date, *aquatic plants*, though by my experiments they seem chiefly concerned in the renovation of the atmosphere, appear to have been too little heeded.

The *Conferva bullosa* receives its specific name from its copious disengagement of oxygenous air-bells. The *Epilobium* is also remarkable in this way; but I know not if I have seen any to compete in that respect with the *Potamogeton verticillatum*. It is in this last copious and uniform.

M. Guy Lussac has found that the air in water contains 32 *per cent.* oxygen, whereas that of our atmosphere exhibits only 21. This is a very important circumstance, and must not be forgotten.

It is admitted that plants as well as animals do, *at night*, evolve carbonic acid gas; but the noxious gas at this period cannot mingle, in either case, with the atmosphere to disturb its salubrity. The gas falls by its own weight, being now chilled, and is finally absorbed by the earth. This check is only required *during the day*, and here vegetation ministers its ample counterpart.

Vegetation, then, is the grand *oxygenemetron* (if I may be allowed to coin a word for the occasion), by which the purity of the atmosphere is preserved inviolate, amid the vicissitude of clime and season. The Great Geometer of the Universe "clothed the lilies of the field," before other animated forms of being took the stations assigned them, in his magnificent and beautiful creation.

ART. XXII.—*Account of a Whirlwind at Roseneath in Dunbartonshire.* By JAMES SMITH, Esq. of Jordanhill, F. R. S. E. In a Letter to Dr BREWSTER.

MY DEAR SIR,

ON the 18th *ult.* a whirlwind of considerable violence took place here; but, fortunately, without doing any material damage. I was from home at the time, but I send you such particulars as I have been able to collect. The day was nearly calm, when a heavy black cloud was observed about a mile to the S. E., nearly at the mouth of the Gare Loch; it had acquired a rotatory motion, accompanied by a violent agitation in the sea under it, the spray rising to a considerable height; and I have no doubt, had it not been interrupted by the point of Roseneath, it would have formed a water-spout. It passed directly over this house, raising up all the loose articles that lay near it, amongst others, an iron coal-skuttle, which lay at the kitchen-door, was lifted about twenty feet, and carried more than a hundred yards into the sea. It broke off several branches from the trees which it passed over, and a boat that lay on the shore, fastened to a tree, was lifted off the ground, and whirled several times round. After passing over the point on which this house stands, it crossed the bay towards the Row Ferry-house. My cutter, which was moored in the bay, was turned several times round; and a sloop that lay near her drove from her anchors. At the Ferry-house it again crossed the land, and lifting up another boat which lay dry, damaged it by its fall. From thence it proceeded up the Gare Loch, and disappeared about two miles above the Ferry. There were several boats near it, but fortunately none came within its influence. The agitated part of the sea appeared to the work people not to exceed the space of an acre, beyond which the sea was as calm as glass. I did not learn whether the barometer was affected. I am always, my dear Sir, yours most truly,

JAS. SMITH.

ROSENEATH,
22d *August* 1822.

ART. XXIII.—*On the Opals of Hungary.* By J. S. BEUDANT, Chevalier of the Legion of Honour, Member of the Wernerian Society, &c.

OPAL, of w [illegible] ary is in some measure the native country, presents [illegible] of more or less remarkable varieties and modificat [illegible] hese varieties, on account of their play of colours, are more or less valued as articles of jewellery; others, of a coarser nature, are not the less important in the eyes of the naturalist, who finds in their variations, their decomposition and their mixtures, the subjects of numerous observations of the first importance for science.

The most celebrated locality for opals is the village named Cservenicza by the Sclavonians (*Vörösvagás*, Hung., pronounced *Tchervenitza* and *Veu-reuche-vagáche*), about two miles from Kaschau (*Kassa*, Hung.), in the trachytic range, which extends from Tokaj to Epériés. It appears that these mines have been wrought for many ages; for Fichtel asserts, that, in the archives of Kaschau, there exist papers which mention that, in the year 1400, there were 300 workmen employed in the country of Cservenitza, as well for the working of opal as for that of mercury. At the present day there are not more than 30, but the working begins to be conducted with regularity.

The varieties which, on visiting these mines, I have found the most abundant, are, the *opaque opal*, of a yellowish or reddish white, and the *milky opal*, which is more or less translucent. The latter becomes sometimes more or less dull, and assumes the characters of certain whitish menilite of the neighbourhood of Paris.

The *fire-opal*, of a beautiful topaz yellow colour, with great lustre, and equally beautiful with that which M. De Humboldt discovered at Mexico, is still pretty common; but the small masses in which it occurs, are very much cracked, and it becomes almost impossible to cut them: it would, however, be a very beautiful stone, were it possible to procure pieces of tolerable size free of fissures. It appears that the yellow colour is owing to iron; for this stone blackens quickly before the blowpipe. It is probable that the metal is here in the state of a hydrate, for it is thus that it occurs in the fissures of the rock where it is deposited by itself.

Limpid opal, without colour, occurs pretty frequently in the interior of small geodes, the mass of which consists of opaque or milky opal; it forms an undulated crust of greater or less thickness, which passes gradually into the preceding layer. Sometimes it occurs by itself in minute fissures; and at other times it is interposed in small layers in the substance of various kinds of opal.

Stalactitic opal (*l'opal concrétionnée*), transparent, translucid, or opaque, is still found in certain fissures of which it lines the walls, or in geodes: these are composed of small stalactites, which adhere more or less firmly to one another, and which, when limpid, differ in no respect from hyalite. They also assume a pearly lustre, when slightly heated, lose their coherence, and break into small scales when exposed to a red heat. Their aggregation gives rise to masses which have the appearance of being homogeneous, and present the aspect of opals similar to some one of the varieties already mentioned, and even to the iridescent opal. Sometimes the stalactites are extremely minute, and completely fill up the irregular cavities; the small fissures by which the siliceous matter has penetrated into the rock being equally filled.

In some instances, the opaline matter has formed on the walls of the fissures only a very thin layer, which presents the appearance of minute tortuous canals, having their surface covered with very minute stalactitiform points lying lengthwise. These surfaces often present the aspect of certain earthy pumices, with elongated and tortuous vesicular cavities.

Iridescent opal (*l'opale irisée*), which is the principal object of research, is also plentifully disseminated in the rocks, but almost always in extremely minute nests; it is very rarely that it occurs in large pieces, like the other varieties. The working sometimes goes on for years before a piece occurs of the size of a twenty sous piece. The largest which has ever been found, in as far as I know, is that of the imperial cabinet of Vienna; it is of the size of one's fist, and weighs 17 ounces. This magnificent specimen has been known at Vienna for more than two centuries, and it is neither known at what period nor how it was obtained. It is polished irregularly to avoid diminishing its size. There are many fissures in it, and it is not completely disengaged from the matrix.

The colours presented by this kind of opal are extremely various; all the [illegible], violet, red, yellow, green, &c. are blended in a t[illegible]nt ways, and present the most brilliant and ag[illegible]s. This beautiful stone is also in great request, and always maintains a high price: the smallest possible, when it is beautiful, does not go for less than four to five guineas; and when the dimensions are large, the value augments altogether beyond proportion. There is a very beautiful specimen at Kaschau, of the size of a small crown-piece (*d'un petit écu*), for which 30,000 florins (or 79,000 francs) were offered. It is reported to have been purchased by the Baron de Brudern, the present concessionary (*concessionaire actuel*) of the opal mines; but I have not learnt for what price.

The iridescent colours presented by opal are not assuredly owing to cracks or fissures, as has sometimes been said; for there is not the least appearance of fissure to be seen in the most beautiful specimens; the smallest fragments into which a piece may be broken, present exactly the same play of light as the largest. This play of light is explained in a less forced manner, by the unequal distribution of vacuities, of different sizes, in which water is found inclosed; it is easy, from the colours observed, to estimate the size, or rather the degree of minuteness of these vacuities, proceeding upon the Newtonian theory of coloured rings.

Ferruginous Opal also occurs pretty frequently in the same veins as the other varieties: it is impregnated with a greater or less quantity of hydrate of iron, which has probably been simultaneously deposited, or into which the siliceous matter may have been subsequently infiltrated. Sometimes the appearance is that of a dull opaque opal, slightly coloured with yellow; but we find, in the same nests, the quantity of iron augmenting by little and little, and often we find nothing but an opal-jasper (*Opal jaspis*, Wern.) similar in every respect to those of which we shall speak afterwards, and which, from this circumstance, appear to be true ferruginous opals. It occasionally occurs in large nests, in which the opal is no longer distinguishable, and to this variety are to be referred the specimens found arranged in collections, under the name of Opal-Jasper of Cservenitza. Sometimes the iron becomes so abundant, that the opaline matter discovers itself only by the resinous lustre which it communicates to the mass. This mixture takes place, as well in the iridescent opal as in the other varieties; it is it which constitutes the black opal (that is to say liver-brown), which it is extremely rare to find possessed of the requisite beauty, because the reflections are destroyed by the mixture: it is also much esteemed on account of its rarity; the reflections are dull, and most commonly violet or purple, all the other tints being absorbed by the colour of the basis.

It is the case that opals of all kinds are susceptible of alteration; they become white, lose their hardness, and may be scratched with the point of a knife; they adhere strongly to the tongue, imbibe water, and, by this means, sometimes regain their transparency and iridescent colours. It is in this state that they have been named Hydrophane; but under this appellation true opals, in an altered state, have been united with calcedonies equally decomposed, and which also regain, when placed in water, more or less of their transparency.

On subjecting these altered opals to various experiments, I found that they also regain their transparence, when heated strongly before the blowpipe; they crack (or split), more or less, at the same time, which seems to indicate a condensation of the substance, and announces, in a different way from that hitherto known, that the opacity is owing to the vacuities contained in the stone. Another observation, which I made at the same time, is, that the opaque varieties, which resume their iridescent colours after immersion in water, do not regain them from the action of fire; they merely become transparent.

Sometimes the alteration and decomposition of opal become complete, and there results a white substance, perfectly earthy, and capable of mixing with water, but not, however, so as to form a paste like argil. This earth is sometimes carried away by the water, and deposited in the cavities which occur in its course.

Independently of these alterations, there exists also, in the most solid and freshest parts of the rock, small nests of a very soft matter, which readily cuts, and produces a particular unctuosity under the edge of the knife. This matter is whitish, yellowish, bluish, and sometimes it presents indications of iridescent reflections. It is very soft to the touch, and, when it has imbibed water, [illegible]fficiently tenacious to be kneaded between the finger [illegible] lieve that this matter is owing to a decomposition [illegible] o that which we have just mentioned, since, from the manner in which it occurs inclosed in the rocks, it could not have been exposed to the influence of the air. I am rather of opinion that it is a particular state of opal. The workmen also distinguish these earthy parts, which they regard as opal that is not yet ripe, from those which are produced by the exposure of opal to the air, which they name burnt or calcined opal. It is to be remarked, in support of the distinction which I have deemed it proper to establish, that these matters harden a little on exposure to the air, and crack in collections, precisely in the same way as alumine or silica, in a state of jelly, which are desiccated in our laboratories. The same circumstance takes place with regard to substances, which approach in character to the opals already described. This I have observed, on the one hand, near Ribnik, in the fissures of semivitreous trachyte; on the other, at Kremnitz, in the cavities of a cellular trachyte, inclosed in conglomerates. It has been, without doubt, observations of this kind which have led certain authors to say, that opals are found, when in the bowels of the earth, so soft as to receive the impression of the fingers, and that they harden only by exposure to the air. This idea is not, perhaps, so ridiculous as might at first be imagined; for we know that silica in solution, assumes, on drying, a certain degree of hardness, and a lustre approaching to that of opal. It is true, that the greater number of opals are solid, when taken from the rock; but, after finding them occasionally still soft, and capable of drying in the air, might it not be supposed that the rest have undergone this desiccation in a slower manner in the bowels of the earth? By admitting this hypothesis, we can discover the reason of the difference which exists between the hyaline quartz and opal; the quartz will be the product of a crystallization of the siliceous matter, and the opal the result of the desiccation of a gelatinous precipitate. I must remark, however, that this is merely a hypothesis, which, while there are some facts in favour of it, has also others against it; such, for example, is the existence of opal stalactites, with regard to which it must be admitted, that the matter has been in a kind of solution.

The rocks in which opal is found sometimes contain sulphuret of iron, in very minute crystals, which I have always found in fissures; and it follows that the opal itself, which is deposited in the same manner, also contains it in greater or less quantity. Thus, pyrites, which appears to belong to the white sulphuret of iron, readily decomposes on exposure to the air, and affords sulphate of iron: it sometimes causes the decomposition of the rock, and even of the opal, in which it is contained. According to my observation, this substance is rather rare, and occurs only here and there, especially in the dark-coloured and soft parts of the matrix (*gangue*). I remember to have heard the existence of pyrites in the opals of Hungary denied. It was asserted, that it only occurs in the varieties of this substance, which are met with in certain metallic veins; but the observations which I have made at Cservenicza, and the specimens which I have brought from thence, place beyond doubt what has already been said by M. Mohs, with regard to this subject. (*Leonard's Taschenbuch*, 7. *année*, *p.* 296.)

Cavities of the porphyritic rock sometimes occur, filled with opals of different kinds, in very thin laminæ, which are very numerous, and separated from one another: these laminæ are sometimes arranged in a parallel manner, and sometimes cross each other at various angles. The aggregate of these laminæ forms a small mass, which has the appearance of being cracked, and which reminds us of the structure of certain nodules of quartz, which sometimes occur in veins, to which the name of Hackly Quartz (*Quarz haché*) has been given.

It is not necessary for me to discuss, in this place, the nature of opal. I shall only mention, that analysis has only discovered silica and water, in the proportion of 1 to 8 or 10. Some chemists have considered this substance as a *hydrate of silica*; but others think that the water is merely interposed: such is the opinion of M. Berzelius, and is founded principally on the fact, that silica cannot be made artificially to combine with water. I cannot speak decidedly in favour of either of these opinions; but I may observe, that it is evident that opal, as well as all that has been designated by the name of Silex Resinite, differs essentially from hyaline quartz, and cannot be considered as being to this substance, what the compact carbonate of lime is to the crystallized carbonate. Opal is very fragile; its fracture in the purest and most transparent parts, is in no respect that of quartz; it resembles the fracture of dried starch, both in the form of its fragments, and in the lustre. Opal is easily scratched by quartz; it is lighter, and the pearly lustre which it assumes on being heated,—the opacity which it acquires,—the facility with which it may then be crumbled down,—the kind of decomposition which it undergoes,—all these circumstances indicate a peculiarity of nature, which, however ill determined as yet, is not the less evident.

It has been erroneously asserted, that opal occurs in Hungary, in successive beds, of many feet in thickness. In all places where I have observed this substance, I have always found it in nests, or rather in veins of greater or less extent, extending in all directions. It is in this manner that it is found at Cservenitza, where this substance is very abundant, and has evidently filled either open fissures in the rock, posteriorly to its consolidation, or irregular cavities naturally existing in it. Not unfrequently, the fissures still open, have their walls covered with a lining of opal of greater or less thickness: in other places, the cavities or cellules filled with stalactites, evidently announce a substance which has been infiltrated; lastly, the veins, which often present vacuities, in which the matter is mamillated, and in some measure purified, still lead to the same result. On examining the specimens, we often perceive the fissures of the rock by which the substance of the opal has penetrated into the cavities which it has filled in part or in whole. In other cases, the manner in which the small nests are disseminated in the matrix, seems to indicate that they have been formed simultaneously with the rock which includes them, and they recal to mind the kidneys of flint which have been formed in our chalks.

The rock in which the nests and veins of opal occur at Cservenitza, has given rise to many opposite opinions: some have considered it as a production of fire; others, as of aqueous origin. Hence it has been named, sometimes a *lava*, sometimes a *granite*, or rather a *porphyry*, an *altered porphyry*, a *brecciated porphyry*, a *hardened clay*. The fact is, that the mass of mountains in which this beautiful substance is found, is entirely formed of trachytic conglomerates, which extend from the heights of Sovar to that of Kaschau, and which present a great number of varieties, according as the fragments of trachytes have been more or less triturated, more or less altered, and as they are more or less numerous in the paste or basis which results from their comminution.

The most elevated parts of these mountains, those which are nearest the centre of the chain, and which immediately rest upon the trachyte *in situ*, are in general composed of large blocks piled upon one another, and between which there exists a finer conglomerate, a coarse paste, resulting from their mutual friction and their decomposition. The more advanced and also lower hills, contain in general smaller and more altered fragments, and the paste is always extremely abundant; it is often confounded with the fragments which it includes; and occurring sometimes absolutely simple in large spaces, it becomes impossible to distinguish the arenaceous nature of the deposit. In this case, we find either homogeneous rocks, with an earthy, smooth, or very large conchoidal fracture, which presents various tints of yellow, red, or brown, or porphyritic rocks, which often even present all the characters of the rocks produced by crystallization. These last varieties are of various natures; some of them present, at first sight, the appearance of a porphyry, because they are marked with a great number of small vesicular cavities, each of which is filled with a white earthy matter. The others have more deceitful characters, because they contain fragments of felspar, or even entire crystals, which often appear to have been formed directly in the earthy mass. These are the rocks which have particularly deceived naturalists regarding the true nature of the matrix of opals, because, on seeing them isolated in collections, it was not possible to know that they are only infinitely small portions in the midst of the mass of very distinct conglomerates. It is not so when they are observed *in situ*. They are seen at the first glance, evidently connected with parts, where the paste, although abundant, incloses very distinct fragments; and hence it is impossible not to remark the intimate connection of these fine deposits with the coarser conglomerates which compose the greater part of the mountains.

The veins or nests of opal occur every where in these masses: they are found [redacted] e surface, as deep in their substance; and nowhere l to observe, as Fichtel asserts, that there exists a sterile upper bed, which must be traversed before the mass containing the opal can be reached; probably the remarks of this naturalist apply only to the iridescent opal, which, as appeared to me, occurs some feet deeper than the surface of the formation. The veins or nests of opaque or milky opal are always more numerous and larger than those of the iridescent variety. Most commonly, these veins traverse indistinctly all the parts of the mass; sometimes the vein bends round blocks or masses of trachyte, and is found placed between them and the paste by which they are agglutinated; sometimes it traverses the blocks themselves, when they are not of great size, or penetrates into their interior, to a certain depth, when their dimensions are very considerable. I have observed, that, in the coarser conglomerate, the veins were in general pretty large, and that there were few nests; while, on the contrary, in the parts where the paste is extremely abundant, I have remarked that the opal occurs more particularly in nests, which appear in some measure cotemporary with the consolidation of the deposit. Sometimes there is even the appearance of the whole mass being consolidated by an opaline siliceous cement, intimately mixed with the earthy parts, and which is deposited, in a pure state, in the small cavities of the rock.

Cservenitza is not the only place where opal is found. It appears that it exists also in the same group of mountains, from Bunnita to Erdöeske, at a little distance to the north-west. It is certain that it has been extracted formerly above *Zamuto*, on the opposite declivity of the mountains. I have besides, found opaque and milky opal in a great number of places where the trachytic conglomerate is abundant; it occurs round Schemnitz and Kremnitz, as well as in the mountains of Matra, and in those of Vihorlet. In short, localities are mentioned as occurring in many places where the rock is still absolutely of the same kind, but nowhere do iridescent opals occur so beautiful and so abundant as in the neighbourhood of Cservenitza.

Although, in general, the trachytic conglomerate appears to be, in Hungary, the particular matrix of the opals, they occur also in other kinds of debris, and even in the rocks *in situ*. I have found the milky variety in the conglomerates of trachytic porphyry, on the sides of the Gran, at Bzenicza, at the extremity of the valley of Eisenbach. It exists also in kidneys in the pumice conglomerate, at *Sajba*, near Neusohl; at *Borfö*, on the southern declivity of the mountains of Schemnitz; at the foot of the mountain of Sator, and at Szanto, near Tallya, in the group of Tokaj; at Erdö-Benye, &c. But the opals of these different places are, in general, less perfectly characterized than those which we have just mentioned; their fracture is intermediate between that of opal, properly so called, and that of flint; they have sometimes a fuliginous tint of colour, and approach to the whitish or brownish menilite of the neighbourhood of Paris; they also pass into opal-jasper, of which we shall speak in the following article.

Opal occurs also in the perlite itself, where it appears in the cellules or fissures of the rock. It is thus that it occurs in nests, and in small veins in the perlite, at the extremity of the Valley of Glasshutte, and at Telkebanya, in the trachytic group of Tokaj. In the first of these localities, the opals are of the whitish opaque, and milky transparent varieties. In the second, they are wax-opals, fire-opals, like those which M. de Humboldt has brought from Zimapan, at Mexico, where they occur in the same matrix. I have never seen this substance in other rocks of the trachytic formation: the siliceous matters contained in the millstone-porphyry (*porphyre molaire*), are always in the state of calcedony, of jasper, or flint.

Opal-Jasper.—Werner has designated by the name of Opal-Jasper a particular species of jasper, the basis of which is of the nature of opal, and which occurs more or less mixed with foreign matters, among which the oxide of iron appears to hold the most important rank. Hungary is still one of the countries where this substance presents itself in the greatest abundance, and that which has furnished the greater number of specimens which we possess in collections. These siliceous matters often fill the fissures of different varieties of trachyte; in other cases, they are found in the form of kidneys, in the midst of altered conglomerates; or, lastly, they have penetrated into wood deposited in the heart of these deposits. They present, in these various circumstances, different degrees of purity, as well as many varieties of colour and general appearance, and pass into each other by a great number of gradations. They are all extremely brittle, and break into a multitude of fragments on the least blow. They are also soft, and may be scratched with great ease by the knife, unless when, being purer and semitransparent, they pass into opal, flint, or common jasper.

The purest opal-jasper, which is only an impure variety of the *Semiopal* of Werner, presents a yellowish or greenish colour; its fracture is sometimes conchoidal, more or less perfect, but most commonly splintery, with the splinters pretty distinctly defined, and the lustre is that of wax. Heated slowly to redness, this kind of opal assumes a brownish colour, which seems to indicate that the colouring matter is oxide of iron.

On the one hand, this variety assumes a little transparency, and then passes into wax-opal, of which it certainly is only a particular state. On the other side, it becomes gradually softer, loses its colour by little and little, and then passes into an impure substance, with an earthy fracture, and a yellowish or greenish white colour, which may be designated by the name of *Earthy Opal-Jasper*. This kind adheres strongly to the tongue; it readily imbibes water, and then assumes a slight semitransparence, and a yellowish or greenish colour, precisely like the wax-opal; it does not blacken in the fire; but when it has been strongly heated, it becomes transparent, and perfectly limpid on the edges, precisely in the same manner as we have seen with regard to the altered opal. These characters, added to the circumstance that this variety always forms the outer part of the kidneys, while, toward the centre, we find the pure waxy opal-jasper, might make us suppose that it is a state produced by decomposition; but this is probably not the case, as we shall show afterwards.

The iron is also found to introduce itself gradually in greater or less quantity, and varieties soon occur which are extremely loaded with it, and which might be designated by the name of *Ferruginous Opal-Jasper*. The colour is deep, of a blackish brown, or chesnut brown, passing sometimes into ochre yellow: these various tints are sometimes united, and form veined or spotted delineations. The fracture is commonly conchoidal, and the lustre is pretty analogous to that of resin. It is this circumstance which has occasionally caused this substance to be called

Pitchstone; although this expression has been exclusively restricted by Werner to vitreous substances, with a somewhat greasy lustre, which, like obsidian, approach to the nature of felspar. The powder of these varieties is rust brown; and when one has broken several specimens, his hands become tinged just in the same way as if he had been touching certain kinds of earthy iron. In some parts of the kidneys, nests occur entirely formed of pulverulent oxide of iron, which are easily detached, and which leave irregular cavities, sometimes divided by partitions of jasper. Sometimes the entire mass is of oxide of iron, or of ochraceous matters, which are found here and there penetrated by the opaline siliceous matter. These masses are, in some parts, sufficiently considerable to be wrought as ores of iron, which is the case, for example, at Kendereske, not far from Munkacs, in the county of Ungh, where the mass is also accompanied with a particular green earth, in which veins of opal-jasper equally occur. The iron ores of *Zamuto* are in the same situation; and, perhaps, also those of Domonya, near Unghvar: it appears that there are similar ones in many other places.

Opal-jasper, whether pure or mixed with oxide of iron, sometimes loses by little and little the resinous lustre which commonly characterises it, and passes, by insensible gradations, into common jasper, which then presents various colours, whitish, yellowish, greenish, blood-red or crimson-red, the fracture being more or less conchoidal. It passes also into calcedony and hornstone. It is also asserted, that beautiful carnelians have occasionally been found in the same places. These varieties are much less abundant than all the others; but they occur precisely in the same matrix, and in the same localities, where they only form separate nests.

We have already seen, that the opals sometimes occur in a soft state. I have not directly observed this state in the opal-jasper; but its occasional existence is sufficiently indicated by many circumstances. In fact, I have often found kidneys or veins of this substance, which presented the characters of a desiccated gel[illegible]us matter, precisely similar to the desiccated gelatinous sil[illegible]oratories. The numerous and large chinks which [illegible]presented in their natural position, those which [illegible]sted in the specimens which I have collected, and which have broken them into small fragments even in my drawers, announce incontestibly a substance which contains much water, and which has undergone a greater or less contraction in proportion as this fluid has evaporated. These circumstances are still in favour of the hypothesis of which I have spoken in the article on Opals; and it seems impossible not to come to the conclusion in a similar case, that a part of the opals and opal-jaspers, are not the result of a gelatinous precipitate of silica, consolidated sometimes by desiccation, sometimes, perhaps, by an infiltration of siliceous matter in a state of solution. This infiltration might easily explain the passages of opal into calcedony; and, at the same time, those characters in some measure intermediate between the two substances, which are so frequently observed. In fact, it may easily be conceived, that a calcedonic liquid may have sometimes penetrated the gelatinous precipitate in greater or less quantity.

The opal-jasper is susceptible of alteration like opal, properly so called, and there result solid substances, capable of imbibing water, and of their regaining a part of their original lustre, or earthy matters which mix with water. But there exists a great difference between the alteration which takes place from exposure to the air, and that which takes place in the interior of the earth. Opal-jasper exposed to the air, even the most ferruginous varieties, becomes entirely white, and similar to hydrophane or cacholong: it appears that the oxide of iron is successively removed in this operation. The stone, nevertheless, preserves a great degree of consistence; it adheres strongly to the tongue; imbibes water, and becomes translucid, without regaining any colour; it remains equally white after having been exposed to a red heat; and, in a clear fire, it becomes perfectly transparent on the edges, as is the case with altered opal. When, on the other hand, decomposition has taken place in the interior of the earth, the mass loses its consistence, and becomes soft to the touch; the oxide of iron constantly remains, and there results a kind of argillaceous matter, of an ochre-yellow colour, which imbibes water, and assumes the original colour of the stone, as well as more or less translucidity. It is to this last kind of decomposition that the state of opal-jasper which we have designated by the name of *earthy*, might be attributed, were we not led to a different conclusion by the nature of the matrix.

The different varieties of opal-jasper occur most commonly in the trachytic conglomerates; and particularly in the more or less altered pumice, where they appear to be formed like flints in the midst of chalk. These kidneys are often precisely defined, and very distinct from the deposit which envelopes them. This is the case with the ferruginous opal-jasper, and with the white opaque opal, into which they pass in a mineralogical sense; but sometimes the kidneys appear to be continuous with the enveloping mass, and to be nothing else than the result of a siliceous infiltration into its substance. This last case presents itself in the kidneys of ferruginous opal-jasper, formed in the midst of deposits of oxide of iron or of argillo-ferruginous matter, and also in the waxy and earthy opal-jasper. These two last varieties form kidneys in the midst of pumicose matters, which have been much altered, extremely divided, and reduced in some parts to an earthy and homogeneous state, These matters, like the earthy jasper, have a yellowish-white or greenish-white colour, and assume, on inbibing water, a deeper tint, as well as a little translucidity. On seeing this position (gisement), it is impossible not to imagine that these very attenuated matters have been posteriorly filled up by siliceous infiltrations, which have consolidated them a little here and there, and given rise to earthy opal-jasper; while, in other parts, becoming more abundant, they formed the waxy opal-jasper. If this mode of formation is not that which has been followed by nature, it must at least be owned that appearances are much in its favour; for the mass of earthy opal-jasper sometimes contains numerous angular cellules, of which some have remained empty, while others are found filled with true yellowish opal, nearly transparent, and of the same tint of colour as the waxy jasper, which is found by its side, in kidneys of greater or less size, and forming part of the same mass. This penetration of the pumicose debris by a siliceous juice, is submitted to examination, in th[illegible] pumice-conglomerates which we have already descri[illegible] has taken place in such large masses, we may, with [illegible], conceive it *a fortiori* to have taken place in [illegible]

Opal-jasper occurs also in veins in the solid rocks of the trachytic formation; but every where that I have met with them in this position, I have always observed trachytic or pumicose conglomerates above them; so that it appeared evident enough that they owed their existence to the fine matters which the waters had drawn off on passing through the conglomerates, and which they had subsequently deposited in the cavities which occurred on their rout. Sometimes they are small veins (filons) of cracked opal-jasper, having the appearance of a desiccated siliceous jelly; sometimes they are veins of oxide of iron, impregnated wholly or in part, with siliceous matter; lastly, they are sometimes fine white matters, which have here and there passed to the state of wax-jasper, or to that of earthy opal-jasper.

It would be useless to point out particular localities for opal-jasper; for it is found every where that pumice-conglomerates exist. I shall content myself with observing here that the village of Sajba, near Neusohl, that of Jasztraba, at a short distance from Kremnitz; and, lastly, that of Borfö, on the southern declivity of the Schemnitz group, are the most celebrated places, and where it appears that beautiful specimens are still procured. Those of Sajba are remarkable for the beautiful colours which they present, and those of Borfö, for the varieties of colour exhibited in the same specimen.

Opalized Wood.—Opaque opal, more or less coloured, and opal-jasper, by impregnating wood buried in the midst of pumicose debris, have given rise to opalized wood (*Holzopal*, Wern.), which, as we know, presents a great variety of colours and degrees of lustre. It is evident that the wood, in this case, has merely served as the receptacle of the siliceous matter which has been successively infiltrated, and in the same manner as it has formed in kidneys in the same conglomerates, or been deposited in the fissures of rocks. The organic nature of the bodies for which the silica has been substituted in this case, has had no sort of influence on the modification which reduces it to the state of opal, since it presents itself with the same characters, in nests or in veins, in the neighbouring parts where no organic debris is found to receive it. A single difference that is observed here, and which still proves that the petrifaction is owing to the infiltration of siliceous matter, is, that this is in some measure pure, and is found disengaged from extraneous matter as by a filter. The greater part also of the opalized wood which I have collected, has a more or less determinate translucidity; the matter is commonly harder, and whatever the colour may be, the powder does not stain the fingers like that of the ferruginous opal-jaspers. Sometimes perfectly transparent opal is observed in the cavities of wood, in small undulated nests, or in stalactites.

The colours which these opalized woods present are extremely numerous; sometimes they are absolutely white, and sometimes they assume very deep colours, yellow, red, brown, green, as well as all the intermediate tints, resulting from the mixture of these principal colours. The same piece of wood, if it be of considerable size, as sometimes happens, presents colours altogether different in their different parts, as well as more or less translucidity or opacity. In many cases the texture of the wood is completely preserved; and when the colour is also retained in an equal degree, it is impossible to distinguish by mere inspection, the petrified specimens from those which are in their original unaltered state. It might be thought that this wood is capable of burning, and I have sometimes been obliged to make the experiment, in order to convince myself that it was not so. In some of the pieces, the tissue of the ash, in others that of the fir, are supposed to be seen; but most commonly it is almost impossible to form any idea of their original nature.

There is also a particular modification in which the wood, although it has passed into the opaline state, presents a fibrous tissue, the fibres in which have no coherence, separate easily from each other, and even preserve their flexibility: not unfrequently a specimen is in one part of this structure, while in another it is compact. It is difficult to say what may be the cause of this modification: I have often imagined that it originated from the circumstance of the wood being in part altered before it became petrified. In fact I have often seen half petrified wood which presented this fil[illegible]e: this explanation, however, is only to be regard[illegible]n.

It is a very remarkable circumstance, that petrified wood, in the state of opal, at least in the numerous specimens which I have seen from many different countries, never presents crystals of quartz in its interior, as is the case with wood in the state of woodstone, and which we find in the newest formations. It seems that, in the former case, the siliceous matter has been modified by some cause hitherto unknown, which has prevented it from crystallizing; while, in the latter case, the molecules have been able to unite symmetrically, and to produce well defined crystals of quartz.

Wood which has passed into a state of opal, occurs particularly in the pumice-conglomerates: it is sometimes in the form of trunks of great size, but more commonly in small pieces. It is difficult to say whether this wood has been silicified in the spots where it now occurs, or has been transported thither subsequently to its petrifaction. However, since opal-jasper exists in the same places in true kidneys, and since the trunks of wood which occur, have no appearance of being rolled, it is probable enough that the place where they are at present found, is that in which they have been impregnated with siliceous matter. Pieces of greater or smaller dimensions are also occasionally found even in fissures of the rocks where opal-jasper is deposited.

I shall not mention any other localities of this opalized wood: it is to be found wherever opal-jaspers occur, that is to say, in all the places where it is formed by deposition from pumicose conglomerates. All the declivities of the trachytic groups which Hungary presents, have furnished it in greater or less quantity; but the most remarkable locality is the village of Sajba, where the most beautiful varieties, whether for colour or for texture, are found. Almost all the specimens in collections have come from this place.

From the details which have been given, we see that every where in Hungary, the opaline siliceous matters, whether pure or mixed, occur particularly in the trachytic conglomerates, or sometimes in infiltrations in the trachytic rocks themselves. Every circumstance tends to confirm the remark, that this is the general position of these substances: in fact, they present themselves in the same manner at Mexico, according to the observations of M. de Humboldt; and in the same way we find in Auvergne opalized wood and opal-jasper in the pumicose conglomerates of Mont Dor and Mont du Cantal. It exists also in the pumice-conglomerates of the banks of the Rhine; it occurs in the Euganean Mountains, in infiltrations in the perlite, and in the felspar rocks which belong to it: it exists equally in the Phlegrean plains; in the Pumice and Lipari Isles, and in the Island of Ischia. The Grecian Isles, *Milo*, *Argentiera*, *Scorgotisa*, and even *Santorin*, contain also a great quantity, which is found also in the conglomerates: of this I have satisfied myself, by examining the collections deposited at Freyberg by M. Hawkins. It appears, therefore, that this modification of the siliceous matter is peculiar to the trachytic formation, just as flint appears to be peculiar to the secondary calcareous, and jasper to primitive or transition rocks. It is thus that the different kinds of minerals appear to be confined each to a particular formation, which is one of the most important facts of geology *.

ART. XXIV.—*On the Alloys of Steel.* By J. STODART, Esq. F. R. S.; and Mr FARADAY, Chemical Assistant to the Royal Institution †.

THE alloys of steel, made on a small scale in the laboratory of the Royal Institution, proving to be good, and the experiments having excited a very considerable degree of interest, both at home and abroad, gave encouragement to attempt the work on a more extended scale, and we have now the pleasure of stating, that alloys similar to those made in the Royal Institution, have been made for the purpose of manufacture; and that they prove to be, in point of excellence, in every respect equal, if not superior, to the smaller productions of the laboratory. Previous, however, to extending the work, the former experiments were carefully repeated, and to the results were added some new combinations, namely, steel with palladium, steel with iridium, and osmium, and latterly, steel with chromium. In this last series of experiments we were particularly fortunate, having by practice acquired considerable address in the management of the furnaces, and succeeded in procuring the best fuel for the purpose. Notwithstanding the many advantages met with in the laboratory of the Royal Institution, the experiments were frequently rendered tedious from causes often unexpected, and sometimes difficult to overcome; among these, the failure of crucibles was perhaps the most perplexing. We have never yet found a crucible capable of bearing the high degree of temperature required to produce the perfect reduction of titanium; indeed, we are rather disposed to question whether this metal has ever been so reduced: our furnaces are equal * (if any are) to produce this effect, but hitherto we have failed in procuring a crucible.

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* From Beudant's Travels.

† From the Philosophical Transactions for 1822. Having already laid before our readers, in vol. iii. p. 308., an account of the early and very interesting experiments of Mr Stodart and Mr Faraday, on the Alloys of Steel with various metals, we avail ourselves of the earliest opportunity of printing the continuation of their valuable experiments. We attempted to make an abstract of this paper, but found the details too important to admit of any curtailment.

Mr Stodart was so obliging as to favour us with specimens of several of these alloys for optical purposes, and though various plates of polished steel kept beside them for more than a year, were all affected with rust, yet not one of the alloys have suffered the least change.—ED.

The metals that form the most valuable alloys with steel are silver, platina, rhodium, iridium and osmium, and palladium; all of these have now been made in the large way, except indeed the last named. Palladium has, for very obvious reasons, been used but sparingly; four pounds of steel, with $\frac{1}{100}$th part of palladium, have however been fused at once, and the compound is truly valuable, more especially for making instruments that require perfect smoothness of edge.

We are happy to acknowledge the obligations due from us to Dr Wollaston, whose assistance we experienced in every stage of our progress, and by whom we were furnished with all the scarce and valuable metals; and that with a liberality which enabled us to transfer our operations from the laboratory of the chemist, to the furnace of the maker of cast steel.

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* We have succeeded in fusing in these furnaces rhodium, and also, though imperfectly, platinum in crucibles.

In making the alloys on a large scale, we were under the necessity of removing our operations from London to a steel furnace at Sheffield; and being prevented by other avocations from giving personal attendance, the superintendence of the work was consequently entrusted to an intelligent and confidential agent. To him the steel, together with the alloying metals in the exact proportion, and in the most favourable state for the purpose, was forwarded, with instructions to see the whole of the metals, and nothing else, packed into the crucible, and placed in the furnace, to attend to it while there, and to suffer it to remain for some considerable time in a state of thin fusion, previous to its being poured out into the mould. The cast ingot was next, under the same superintendence, taken to the tilting-mill, where it was forged into bars of a convenient size, at a temperature not higher than just to render the metal sufficiently malleable under the tilt hammer. When returned to us, it was subjected to examination both mechanical and chemical, as well as compared with the similar products of the laboratory. From the external appearance, as well as from the texture of the part when broken by the blow of the hammer, we were able to form a tolerably correct judgment as to its general merits: the hardness, toughness, and other properties, were farther proved by severe trials, after being fashioned into some instrument, or tool, and properly hardened and tempered.

It would prove tedious to enter into a detail of experiments made in the Royal Institution; a brief notice of them will at present be sufficient. After making imitations of various specimens of meteoric iron by fusing together pure iron and nickel, in proportions of 3 to 10 per cent., we attempted making an alloy of steel with silver, but failed, owing to a superabundance of the latter metal. It was found, after very many trials, that only the $\frac{1}{500}$th part of silver would combine with steel, and when more was used a part of the silver was found in the form of metallic dew lining the top and sides of the crucible. The fused button itself was a mere mechanical mixture of the two metals, globules of silver [illegible] ressed out of the mass by contraction in cooling, and [illegible] lobules being forced out by the hammer in forgi[illegible] r, when the forged piece was examined, by dis[illegible] iluted sulphuric acid, threads or fibres of silver were seen mixed with the steel, having something of the appearance of steel and platina when united by welding: but when the proportion of silver was only $\frac{1}{500}$th part, neither dew, globules, nor fibres appeared, the metals being in a state of perfect chemical combination, and the silver could only be detected by a delicate chemical test.

With *platina* and *rhodium*, *steel* combines in every proportion; and this appears also to be the case with iridium and osmium: from 1 to 80 per cent. of platina was perfectly combined with steel, in buttons of from 500 to 2000 grains. With rhodium, from 1 to 50 per cent. was successfully used. Equal parts by weight of steel and rhodium, gave a button, which, when polished, exhibited a surface of the most equisite beauty: the colour of this specimen is the finest imaginable for a metallic mirror, nor does it tarnish by long exposure to the atmosphere: the specific gravity of this beautiful compound is 9.176. The same proportion of steel and platina gave a good button, but a surface highly crystalline renders it altogether unfit for a mirror. In the laboratory we ascertained that, with the exception of silver, the best proportion of the alloying metal, when the object in view was the improvement of edge-tools, was about $\frac{1}{100}$th part, and in this proportion they have been used in the large way. It may be right to notice, that, in fusing the metals in the laboratory, no flux whatever was used, nor did the use of any ever appear to be required.

Silver being comparatively of little value with some of the alloying metals, we were disposed to make trial with it as the first experiment in the large way. 8 lb. of very good Indian steel was sent to our agent, and with it $\frac{1}{500}$th part of pure silver: a part of this was lost owing to a defect in the mould; a sufficient quantity was however saved, to satisfy us as to the success of the experiment. This, when returned, had the most favourable appearance both as to surface and fracture: it was harder than the best cast-steel, or even than the Indian wootz, with no disposition whatever to crack, either under the hammer, or in hardening. Some articles, for various uses, have been made from this alloy; they prove to be of a very superior quality. Its application will probably be extended not only to the manufacture of cutlery, but also to various descriptions of tools; the trifling addition of price cannot operate against its very general introduction. The silver alloy may be advantageously used for almost every purpose for which good steel is required.

Our next experiment made in the large way, was with *steel* and *platina*. 10 lb. of the same steel, with $\frac{1}{100}$th part of platina, the latter in the state produced by heating the ammonia muriate in a crucible to redness, was forwarded to our agent, with instructions to treat this in the same way as the last named metals. The whole of this was returned in bars remarkable for smoothness of surface and beauty of fracture. Our own observation, as well as that of the workmen employed to make from it various articles of cutlery, was, that this alloy, though not so hard as the former, had considerably more toughness: this property will render it valuable for every purpose where tenacity, as well as hardness, is required; neither will the expence of platina exclude it from a pretty general application in the arts; its excellence will much more than repay the extra cost.

The alloys of steel with rhodium have also been made in the large way, and are perhaps the most valuable of all; but these, however desirable, can never, owing to the scarcity of the metal, be brought into very general use. The compound of steel, iridium and osmium, made in the large way, is also of great value; but the same cause, namely, the scarcity and difficulty of procuring the metals, will operate against its very general introduction. A sufficient quantity of these metals may perhaps be obtained to combine with steel for the purpose of making some delicate instruments, and also as an article of luxury, when manufactured into razors. In the mean time we have been enabled, repeatedly to make all these alloys (that with palladium excepted) in masses of from 8lb. to 20lb. each; with such liberality were we furnished with the metals from the source already named.

A point of great importance in experiments of this kind was, to ascertain whether the products obtained were exactly such as we wished to pr [illegible] For this purpose, a part of each product was analy [illegible] ome cases the quantity ascertained; but it was not [illegible] essary in every case to verify the quantity by an [illegible] in all the experiments made in the laboratory, the button produced after fusion was weighed, and if it fell short of the weight of both metals put into the crucible, it was rejected as imperfect, and put aside. When the button gave the weight, and on analysis gave proofs of containing the metal put in to form the alloy, and also on being forged into a bar and acted on by acids, presented an uniform surface, we considered the evidence of its composition as sufficiently satisfactory. The processes of analysis, though simple, we shall briefly state; the information may be desirable to others who may be engaged on similar experiments; and, farther, may enable every one to detect any attempt at imposition. It would be very desirable at present, to possess a test as simple, by which we could distinguish the wootz, or steel of India, from that of Europe; but this, unfortunately, requires a much more difficult process of analysis.

To ascertain if platina is in combination with steel, a small portion of the metal, or some filings taken from the bar, is to be put into dilute sulphuric acid; there will be rapid action; the iron will be dissolved, and a black sediment left, which will contain carbon, hydrogen, iron, and platina; the carbon and hydrogen are to be burnt off, the small portion of iron separated by muriatic acid, and the residium dissolved in a drop or two of nitro-muriatic acid. If a piece of glass be moistened with this solution, and then heated by a spirit-lamp and the blowpipe, the platina is reduced, and forms a metallic coating on the glass.

In analysing the alloy of *steel* and *silver*, it is to be acted on by dilute sulphuric acid, and the powder boiled in the acid; the silver will remain in such a minute state of division, that it will require some time to deposit. The powder is then to be boiled in a small portion of strong muriatic acid *; this will dissolve the iron and silver, and the latter will fall down as a chloride of silver on dilution with water; or the powder may be dissolved in pure nitric acid, and tested by muriatic acid and ammonia.

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* Although it is a generally received opinion that muriatic acid does not act on silver, yet that is not the case; pure muriatic acid dissolves a small portion of silver very readily.

The alloy of steel and palladium, acted on by dilute sulphuric acid, and boiled in that acid, left a powder which, when the charcoal was burnt from it, and the iron partly separated by cold muriatic acid, gave on solution in hot muriatic acid, or in nitro-muriatic acid, a muriate of palladium. The solution, when precipitated by prussiate of mercury, gave prussiate of palladium; and a glass plate moistened with it and heated to redness, became coated with metallic palladium.

The residuum of the rhodium alloy obtained by boiling in diluted sulphuric acid, had the combustible matter burnt off, and the powder digested in hot muriatic acid: this removed the iron; and by long digestion in nitro-muriatic acid, a muriate of rhodium was formed, distinguishable by its colour, and by the triple salt it formed with muriate of soda.

To analyse the compound of *steel* with *iridium* and *osmium*, the alloy should be acted on by dilute sulphuric acid, and the residium boiled in the acid: the powder left is to be collected and heated with caustic soda in a silver crucible to dull redness for a quarter of an hour, the whole to be mixed with water, and having had excess of sulphuric acid added, it is to be distilled, and that which passes over condensed in a flask: it will be a solution of oxide of osmium; will have the peculiar smell belonging to that substance, and will give a blue precipitate with tincture of galls. The portion in the retort being then poured out, the insoluble part is to be washed in repeated portions of water, and then being first slightly acted on by muriatic acid, to remove the iron, is to be treated with nitro-muriatic acid, which will give a muriate of iridium.

In these analyses, an experienced eye will frequently perceive, on the first action of the acid, the presence of the alloying metal. When this is platina, gold, or silver, a film of the metal is quickly formed on the surface of the acid.

Of alloys of platina, palladium, rhodium, and iridium and osmium, a ready test is offered when the point is not to ascertain what the metal is, but merely whether it be present or not. For this purpose, we have only to compare the action of the same acid on the all [illegible] piece of steel; the increased action on the alloy ir [illegible] cates the presence of the metal; and by the dif [illegible], which, on experience, is found to be produced with the different metals, a judgment may be formed even of the particular one present.

The order in which the different alloys stand with regard to this action, is as follows: steel, cromium alloy, silver alloy, gold alloy, nickel alloy, rhodium alloy, iridium and osmium alloy, palladium alloy, platina alloy. With similar acid the action on the pure steel was scarcely perceptible; the silver alloy gave very little gas, nor was the gold much acted on. All the others gave gas copiously, but the platina alloy in most abundance.

In connection with the analysis of these alloys, there are some very interesting facts to be observed during the action of acids on them, and perhaps none of these are more striking than those last referred to. When the alloys are immersed in diluted acid, the peculiar properties which some of them exhibit, not only mark and distinguish them from common steel, and from each other, but also give rise to some considerations on the state of particles of matter of different kinds, when in intimate mixture or in combination, which may lead to clearer and more perfect ideas on this subject.

If two pieces, one of steel, and one of steel alloyed with platina, be immersed in weak sulphuric acid, the alloy will be immediately acted on with great rapidity and the evolution of much gas, and will shortly be dissolved, whilst the steel will be scarcely at all affected. In this case, it is hardly possible to compare the strength of the two actions. If the gas be collected from the alloy and from the steel for equal intervals of time, the first portions will surpass the second some hundreds of times.

A very small quantity of platina alloyed with steel, confers this property on it: $\frac{1}{400}$ increased the action considerably; with $\frac{1}{200}$ and $\frac{1}{100}$ it was powerful; with 10 per cent. of platina it acted, but not with much power; with 50 per cent. the action was not more than with steel alone; and an alloy of 90 platina with 20 steel, was not affected by the acid.

The action of other acids on these alloys, is similar to that of sulphuric acid, and is such as would be anticipated: dilute muriatic acid, phosphoric acid, and even oxalic acid, acted on the platina alloy with the liberation of more gas than from the zinc; and tartaric acid and acetic acid rapidly dissolved it. In this way chalybeate solutions, containing small portions of protoxide of iron, may be readily obtained.

The cause of the increased action of acids on this and similar alloys, is, as the President of this Society suggested to us, probably electrical. It may be considered as occasioned by the alloying metal existing in such a state in the mass, that its particles form voltaic combinations with the particles of steel, either directly, or by producing a definite alloy, which is diffused through the rest of the steel; in which case, the whole mass would be a series of such voltaic combinations; or it may be occasioned by the liberation, on the first action of the acid, of particles which, if not pure platina, contain, as has been shewn, a very large proportion of that metal, and which, being in close contact with the rest of the mass, form voltaic combinations with it in a very active state: or, in the third place, it may result from the iron being mechanically divided by the platina, so that its particles are more readily attacked by the acid, analogous to the case of proto-sulphuret of iron.

Although we have not been able to prove by such experiments, as may be considered strictly decisive, to which of these causes the action is owing, or how much is due to any of them, yet we do not hesitate to consider the second as almost entirely, if not quite, the one that is active. The reasons which induce us to suppose this to be the true cause of the action, rather than any peculiar and previous arrangement of the particles of steel and platina, or than the state of division of the steel, are, that the two metals combine in every proportion we have tried, and do not, in any case, exhibit evidences of a separation between them, like those, for instance, which steel and silver exhibit; that when, instead of an acid, weaker agents are used, the alloy does not seem to act with them as if it was a series of infinitely minute voltaic combinations of steel and platina, but exactly as steel alone would do; that the mass does not render platina wire more negative than steel, as it probably in the third case would do; that it does not rust more rapidly in a damp atmosphere; and that when placed in saline solutions, as muriate of soda, &c., [illegible] takes place between them. In such cases it acts ju[illegible] and no agent that we have as yet tried, has produced voltaic action that was not first able to set a portion of the platina free by dissolving out the iron.

Other interesting phenomena exhibited by the action of acids on these steels, are the differences produced when they are hard and when soft. Mr Daniel, in his interesting paper on the mechanical structure of iron, published in the Journal of Science, has remarked, that pieces of hard and soft steel being placed in muriatic acid, the first required fivefold the time of the latter to saturate the acid; and that when its surface was examined, it was covered with small cavities like worm-eaten wood, and was compact and not at all striated, and that the latter presented a fibrous and wavy texture.

The properties of the platina alloy, have enabled us to observe other differences between hard and soft steel equally striking. When two portions of the platina alloy, one hard and one soft, are put into the same diluted sulphuric acid, and suffered to remain for a few hours, then taken out and examined, the hard piece presents a covering of a metallic black carbonaceous powder, and the surface is generally slightly fibrous; but the soft piece, on examination, is found to be covered with a thick coat of grey metallic plumbaginous matter, soft to the touch, and which may be cut with a knife, and its quantity seven or eight times that of the powder on the hard piece: it does not appear as if it contained any free charcoal, but considerably resembles the plumbaginous powder Mr Daniel describes as obtained by the action of acid on cast iron.

The same difference is observed if pure steel be used, but it is not so striking; because, being much less rapidly attacked by the acid, it has to remain longer in it, and the powder produced is still farther acted on.

The powder procured from the soft steel or alloy in these experiments, when it has not remained long in the acid, exactly resembles finely divided plumbago, and appears to be a carburet of iron, and probably of the alloying metal also. It is not acted on by water, but in the air the iron oxidates and discolours the substance. When it remains long in the acid, or is boiled in it, it is reduced to the same state as the powder from the hard steel or alloy.

When any of these residua are boiled in diluted sulphuric or muriatic acid, protoxide of iron is dissolved, and a black powder remains unalterable by the farther action of the acid. It is apparently in greater quantity from the alloys than from pure steel, and when washed, dried, and heated to 300° or 400° in the air, burns like pyrophorus, with much fume; or if lighted, burns like bitumen, and with a bright flame: the residuum is protoxide of iron, and the alloying metal. Hence, during the action of the acid on the steel, a portion of hydrogen enters into combination with part of the metal and the charcoal, and forms an inflammable compound not acted upon by the acid.

Some striking effects are produced by the action of nitric acid on these powders. If that from pure steel be taken, it is entirely dissolved; and such is also the case if the powder be taken from an alloy, the metal of which is soluble in nitric acid; but if the powder is from an alloy, the metal of which is not soluble in nitric acid, then a black residuum is left not touched by the acid; and which, when washed and carefully dried, is found, when heated, to be deflagrating; and with some of the metals, when carefully prepared, strongly explosive.

The fulminating preparation obtained from the platina alloy, when dissolved in nitro-muriatic acid, gave a solution containing much platina, and very little iron. When a little of it was wrapped in foil and heated, it exploded with much force, tearing open the foil, and evolving a faint light. When dropped on the surface of heated mercury, it exploded readily at 400° of Fahrenheit, but with difficulty at 370°. When its temperature was raised slowly, it did not explode, but was decomposed quietly. When detonated in the bottom of a hot glass tube, much water and fume were given off, and the residuum collected was metallic platina, with a very little iron and charcoal. We are uncertain how far this preparation resembles the fulminating platina of Mr Edmund Davy.

In these alloys of steel the differences of specific gravity are not great, and may, probably, be in part referred to the denser state of the metals from more or less hammering; at the same time it may be [illegible] at they are nearly in the order of the specific graviti[illegible] tive alloying metals.

The alloys of steel with gold, tin, copper, and chromium, we have not attempted in the large way. In the laboratory, steel and gold were combined in various proportions; none of the results were so promising as the alloys already named, nor did either tin or copper, as far as we could judge, at all improve steel. With titanium we failed, owing to the imperfection of crucibles. In one instance, in which the fused button gave a fine damask surface, we were disposed to attribute the appearance to the presence of titanium; but in this we were mistaken; the fact was, we had unintentionally made wootz. The button, by analysis, gave a little silex and alumine, but not an atom of titanium; menachanite, in a particular state of preparation, was used: this might possibly contain the earths or their bases, or they may have formed a part of the crucible.

M. Berthier, who first made the alloy of steel and chromium*, speaks very favourably of it. We have made only two experiments. 1600 grains of steel, with 16 of pure chrome, were packed into one of the best crucibles, and placed in an excellent blast furnace: the metals were fused, and kept in that state for some time. The fused button proved good and forged well: although hard, it showed no disposition to crack. The surface being brightened, and slightly acted on by dilute sulphuric acid, exhibited a crystalline appearance; the crystals being elongated by forging, and the surface again polished, gave, by dilute acid, a very beautiful damask. Again, 1600 grains of steel, with 48 of pure chrome, were fused: this gave a button considerably harder than the former. This, too, was as malleable as pure iron, and also gave a very fine damask. Here a phenomenon rather curious was observed: the damask was removed by polishing, and restored by heat without the use of any acid. The damasked surface, now coloured by oxidation, had a very novel appearance: the beauty was heightened by heating the metal in a way to exhibit all the colours caused by oxidation, from pale straw to blue, or from about 430° to 600° of Fahrenheit. The blade of a sabre, or some such instrument, made from this alloy, and treated in this way, would assuredly be beautiful, whatever its other properties might be; for of the value of the chrome alloy for edge tools we are not prepared to speak, not having made trial of its cutting powers. The sabre blade thus coloured, would amount to a proof of its being well tempered; the blue black would indicate the temper of a watch spring, while the straw-colour towards the edge would announce the requisite degree of hardness. It is confessed, that the operation of tempering any blade of considerable length in this way, would be attended with some difficulty.

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* Annales de Chimie, xvii. 55.

In the account now given of the different alloys, only one triple compound is noticed, namely, steel, iridium, and osmium; but this part of the subject certainly merits farther investigation, offering a wide and interesting field of research. Some attempts to form other combinations of this description proved encouraging; but we were prevented, at the time, by various other avocations, from bestowing on them that attention and labour they seemed so well to deserve [*].

It is a curious fact, that when pure iron is substituted for steel, the alloys so formed are much less subject to oxidation. 3 *per cent.* of iridium and osmium fused with some pure iron, gave a button, which, when forged and polished, was exposed, with many other pieces of iron, steel, and alloys, to a moist atmosphere: it was the last of all showing any rust. The colour of this compound was distinctly blue; it had the property of becoming harder, when heated to redness and quenched in a cold fluid. On observing this steel-like character, we suspected the presence of carbon; none, however, was found, although carefully looked for. It is not improbable that there may be other bodies, besides charcoal, capable of giving to iron the properties of steel; and though we cannot agree with M. Boussingault [†], when he would replace carbon in steel by silica or its base, we think his experiments very interesting on this point, which is worthy of farther examination.

We are not informed as to what extent these alloys, or any of them, have been made at home, or to what uses they have been applied: their more general introduction in manufacture of cutlery would assuredly add to the value, and consequently to the extension of that branch of trade. There are various other important uses to which the alloys of steel may advantageously be applied. If our information be correct, the alloy of silver, as well as that of platina, has been, to some considerable extent, in use at his Majesty's mint. We do know, that several of the alloys have been diligently and successfully made on the Continent, very good specimens of some of them having been handed to us; and we are proud of these testimonies of the utility of our endeavours.

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[*] It is our int [illegible] e these experiments at every opportunity; but they are laboriou [illegible] time and patience.

[†] Annales de [illegible]

To succeed in making and extending the application of these new compounds, a considerable degree of faithful and diligent attention will be required on the part of the operators. The purity of the metals intended to form the compound is essential; the perfect and complete fusion of both must, in every case, be ascertained: it is farther requisite, that the metals be kept for some considerable time in the state of thin fusion; after casting, the forging is with equal care to be attended to; the metal must, on no account, be overheated; and this is more particularly to be attended to, when the alloying metal is fusible at a low temperature, as silver. The same care is to be observed in hardening: the article is to be brought to a cherry-red colour, and then instantly quenched in the cold fluid.

In tempering, which is best performed in a metallic bath properly constructed, the bath will require to be heated for the respective alloys, from about 70° to 100° of Fahrenheit above the point of temperature required for the best cast-steel. We would farther recommend, that this act of tempering be performed twice; that is, at the usual time before grinding, and again just before the last polish is given to the blade. This second tempering may perhaps appear superfluous, but upon trial its utility will be readily admitted. We were led to adopt the practice by analogy, when considering the process of making and tempering watch springs.

Art. XXV.—*History of Mechanical Inventions and Processes in the Arts.*

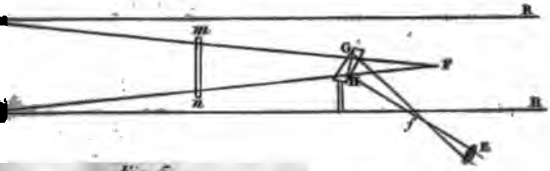
1. *The* Rev. Mr Cecil's *Machine, in which Hydrogen Gas is the Moving Power.*

In the second part of the first volume of the *Transactions of the Cambridge Philosophical Society*, there is a paper . "On the Application of Hydrogen Gas to produce a Moving Power," with a description of a machine in which it may be employed.

The general principle seems to be, that a mixture of one portion of hydrogen gas, with two and a half portions of common air, on being exploded, will expand to three times its bulk, and then instantly collapse to one-sixth of its original volume. If, therefore, a cylindric vessel be provided, separated at one-third of its length into two portions, by a valve moving round an axis in the line of its diameter (commonly called a throttle-valve) ; if there be a solid piston in the shorter portion, and if, at the extremity of the longer portion, there be a *light* valve opening easily outward, then, supposing the throttle-valve shut, and the piston close down to it, if the piston be drawn up, and the mixture of air and hydrogen be allowed to flow in to supply the space left by it, until it reach the end of its stroke, if at this time the valve be opened, and at the same time a jet of flame be made to communicate with the gaseous mixture (by a touch-hole in the side of the cylinder), then an explosion will take place, which will drive out the common air from the larger portion of the cylinder by the end valve, which will clap to on the condensation taking place, and the pressure of the atmosphere into the rare medium now in the cylinder, will force down the piston until it reaches nearly to the middle valve, when the gas is again admitted, the middle valve closed, and the end one opened : the ascent of the piston must be operated by the inertia of a fly-wheel, put into motion by the downward stroke.

This is somewhat like the process which goes on in a Newcomen's engine. The atm[illegible] is the moving power in both, and the steam and cold w[illegible]he one, perform the same offices as the hydrogen and [illegible] in the other.

Fig. 1.



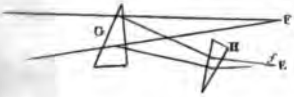


Fig. 5.

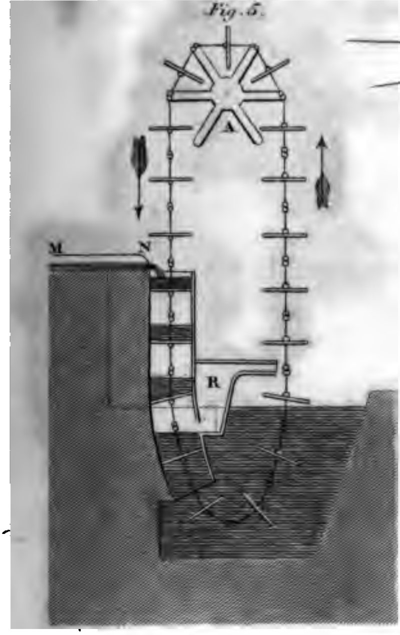


Fig. 4.

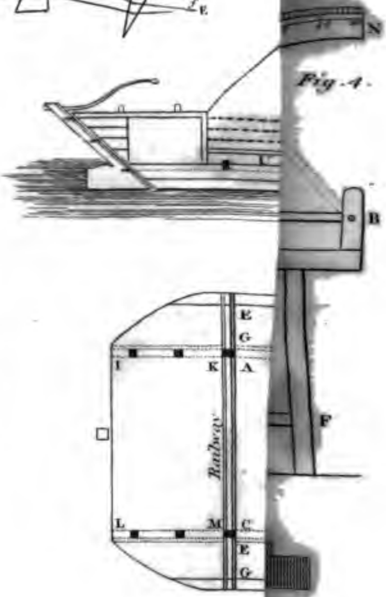


Fig. 7.

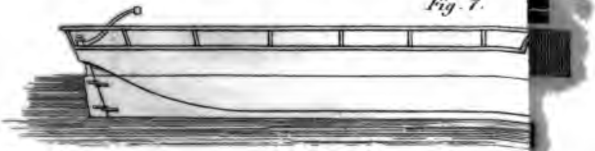
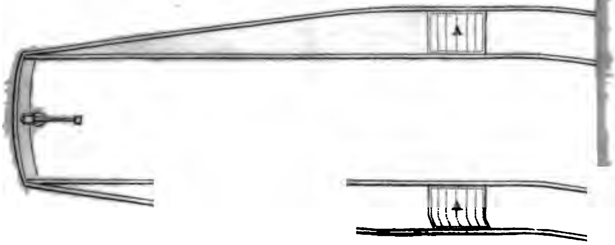


Fig. 6.



It will be observed, that no advantage is taken of the expansive force of the explosion, except to drive away the common air from the lower part of the cylinder. The construction of such machines admits of endless modifications; but the expence of the hydrogen gas appears to be a prohibition to its employment, except in some rare situations.

2. *Account of Mr Bald's Steam Passage-Boat for the Alloa Ferry.*

We have much pleasure in laying before our readers the following account of a very excellent Steam Passage-Boat, which has been proposed by Mr Bald, for facilitating the transit of passengers, cattle and carriages, over the river Forth, at Alloa Ferry *. The plan displays much ingenuity and knowledge, and will, we doubt not, be followed at many other ferries.

" The passage-boat is represented in Plate V. Figs. 2. and 3., where,

A.B.C.D.	Are areas where cattle stand.
E.F.G.H.	Areas connecting the wings where carriages stand, with horses attached.
I.K.L.M.	Poops for helmsman, and cabin for passengers.
N.	Paddles.
O.	Cylinder of steam-engine.
P.P.	Chimneys.
Q.Q.	Boilers.
R.	Coal-house.
S.	Festoon chains to hook and unhook similar chains, and keep the cattle within the platforms A.B.C.D.
T.	The wings.
U.	Folding guard boards.

The extreme length of the boat upon deck to be about		82 feet.
Breadth over all upon deck, - -	20 feet.	
Additional for two wings five feet each, -	10	
Breadth over wings, - - -	—	30

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* Mr Rennie estimated the expence of a bridge at L. 60,000. The breadth of the river is 570 yards.

The space for engine in centre of boat, -	17
Platform for cattle, &c. at one end, - -	20
Platforms for cattle, &c. at the other end, -	20 feet.
Poop or cabin at the one end, - - -	12 : 6 in.
Do. at other end, - - - -	12 : 6

The bottom of the boat to be quite flat, without a keel. The sides to be at right angles to the bottom.

The framing to be of oak or beech floors, with oak or larch wood knees.

The bottom deals to be of beech, with beech bilge planks eighteen inches up the side.

The rest of the plank and deck to be of red pine-wood. Each end of the boat to be formed alike, after the scow-form, with a rudder at both ends, so that the boat never requires to be turned, and the rudders so contrived as to be easily drawn up clear of the water. As it is found that horses, in particular those which are high bred, are very restive when they go on deal boards, it is proposed to obviate this inconveniency, by covering the whole of the platforms and wings opposite to them, with old junk, which will be soft and yielding, somewhat like a common road.

By this arrangement, four chaises, with their horses attached, or as many carriers' carts, with their loading, can be taken upon the platforms at once.

The passengers can have accommodation either in the poops or on the wings, as they may choose; the entry to the poops or cabins being by the side.

The depth of the boat to be 4 feet, and at the poop and engine-house, 8 feet. When the boat is fully loaded, it is thought she will only draw about 2 feet water. The deck of the platforms to be placed at 2 feet 6 inches from the bottom of the boat, with folding down guard-boards, 18 inches deep, divided lengthways into two equal spaces, and folding upon a stanchion in the middle of the space. These guard-boards fold down upon the pier, when cattle or carriages are going out of the boat, and may be made 3 [illegible] depth, if this is thought an improvement.

When cattle [illegible] over, they are to be confined within the breadth of [illegible] is, they are not to stand upon the wings. They will therefore be confined to that space, by three tier of chains, which hook and unhook at pleasure; and a similar set of chains are stretched in the line of the wings along the side of the platforms, as represented in the drawing.

With regard to the power of the steam-engine, it must be kept in view, that the run of the tide at this Ferry, in particular the ebb at the South Pier, is uncommonly rapid; and this, when aided by the strong prevailing south-west wind in the direct line of the Pier, evidently requires considerable power to propel so large a boat with her loading. On the other hand, the boat being flat-bottomed, without a keel, and drawing only 2 feet water, the resistance of the water is very little, compared with other steam-boats.

Mr Napier is of opinion, that an engine of twelve horses power will be quite sufficient; but, in order to remove every dubiety about making the passage in all weathers, an engine of sixteen or eighteen horses power may be adopted.

Mr Napier proposed that the engine should be constructed with the engine-beam working over the cylinder; but, in order to throw the centre of gravity as near the bottom of the boat as possible, I think it would be preferable to have two beams placed near the bottom of the condensing cistern, after the manner of those in the Morning Star Steam-boat of Alloa.

With regard to the expence of a boat with an engine of twelve horses power, Mr Napier estimates that it will cost L. 1200; and for every additional horse's power from L. 40 to L. 50 more.

From the construction of the boat, it is probable, that, when a heavy loaded cart is resting upon the wings, either in going in or out of the boat, she may heel considerably with the weight; and, when the carts, with their loadings, are in their position, the boat may not float upon a horizontal floor. To obviate this, I propose that an iron railway be laid across the boat, upon each poop next the platform; upon which railway, two wheeled carriages, or trucks, may be placed, with about a ton of iron in each. These can, with the greatest ease, be moved from side to side, to produce an equipoise, and firmly secured at any particular point.

From the heavy running ebb-tide, and the prevailing strong south-westerly winds, a difficulty occurs, in such cases, in laying the boat safely alongside of the South Pier. To obviate this, I propose that there be placed two flat mooring buoys, in a line with the South Pier, and at such distance from each other as may best suit the ebb-tide. These buoys will float horizontally; and, from the centre, there will be a strong upright iron rod of about 10 feet in height, with an eye or ring at top. From each of these rings a strong chain will proceed to the pier, hung in a festoon form, with some spare chain lying upon the Pier.

During the first run of the ebb, the lower chain will be allowed to go down to the bottom of the water, that the boat may pass over it. The boat will then be directed to the upper buoy, when, the steam-engine being stopped, the boatmen will take a turn of the chain round one of the timber heads, and ease it off, till she fall slowly alongside of the Pier. This will prevent all injury arising from sudden concussion against the masonry. At half ebb the other chain will be used in a similar manner.

These mooring-buoys will also be of use in swinging the boat clear of the Pier, during the heavy run of the ebb-tide.

There is an obstruction of a formidable nature, regarding the passage being made during winter, in long and continued severe frost, when the river is often covered from side to side with large floating shoals of ice. To obviate this, I would propose that the sides of the boat should be sheathed with sheet-iron, such as is used for the ice-boats of the Forth and Clyde Canal; that the paddle-boards should be of strong iron in place of wood; and that at each end of the boat there should be a number of stampers to be wrought by the engine. These, I am of opinion, would easily and completely break the ice; and the power of the engine would propel the boat, so as to make the passage generally at all times.

If, however, it shall happen, that, during very severe storms of continued frost, a passage cannot be effected by this means, passages can be made always at high-water, as at this time all the ice has pass[illegible]rds.

I conclude t[illegible]f forcing a passage during the continuance of the [illegible]er, may be effected by the means proposed, because we know that the ice-boat upon the great canal before mentioned, with the power of a few horses, breaks the very solid ice of the canal, where it forms a firm compact sheet from bank to bank.

I need not enter upon all the various advantages which will arise from the establishment of this steam-boat: they are obvious to every one who is acquainted with this district of country. At present I cannot give an exact account of the distance which will be saved in travelling by this route from Perth to Glasgow. The road, however, will be shortened at least from twelve to fifteen miles, which is of great importance to travellers.

According to my view, the steam-boat will ply summer and winter, from morning light till dusk; the small boats and crew being always ready for crossing the river during the night, in case of emergency, or during the repairs of the engine in the day.

"I estimate the weekly expence of the steam-boat to be as follows:

Engineer, - - - - -	L. 1	1	0
Four boatmen 7 days, at 2s. 6d. - -	3	10	0
No fireman.			
Total wages, - - -	L. 4	11	0
Coals, 2 tons *per* day, at 6s., - -	4	4	0
Tallow, oil, &c. *per* week, - -	1	5	0
Expence *per* week, - - -	L. 10	0	0"

3. Mr HARFORD's *Improvements on Puddling Furnaces.*

In that department of the manufacture of iron called *puddling*, cast-iron bottoms or floors have been generally employed, but as these bottoms decay rapidly, and as it has been found that the iron slag, scoria or sand, which are employed to defend these bottoms from injury, impart impurities to the iron, Mr Harford has adopted the following mode of rendering them durable, which he has secured by patent. He spreads over the cast-iron bottoms a quantity of charcoal, reduced to powder, which being a bad conductor of heat protects the cast-iron floors better than any other substance, from the intense heat which is required in these furnaces. This very simple contrivance is said to produce iron of a very superior quality.

4. *Description of a Ductilimeter for comparing the ductility of different Metals.* By M. Regnier.

This instrument, which is represented in Plate V. Fig. 4., is intended for ascertaining and comparing the ductility of different fusible metals, as *lead, tin*, &c. It consists of a hammer, AB, made of iron, and moving round a horizontal axle B, a wooden quadrant MN fixed in the wooden frame EF, and a plate of iron C, on which is fixed the ball of lead or tin. The lead or tin is fused into regular balls, about 10 millimetres in diameter, and each ball is laid upon the centre of several concentric circles engraven on the iron plate. The hammer AB is then raised to the 50th degree of the arc MN, and is allowed to descend upon the balls. By successive blows, the balls are enlarged to circular discs, 30 millimetres in diameter, and the number of blows necessary for this purpose will give a measure of the comparative ductilities, the following are some of the results obtained by M. Regnier :

	Blows of the hammer.
Old lead remelted 10 times, - - -	10
New lead of Sir W. Blackett, - - -	11
Do. of Caldebek, - - -	12
Lead containing a tenth part of zinc, - -	14
Tin of Cornwall, - - -	40

See the *Annales des Mines*, tom. vii. p. 13.

5. *On the Fabrication of Artificial Pastes in Imitation of Precious Stones.* By M. Douault Wieland.

A prize having been offered by the Society for the Encouragement of the Arts in France for the best memoir on the subject, it was decreed to M. D. Wieland.

The base of all these imitations is Strass, or White Crystal. The materials employed are melted in Hessian crucibles, and a porcelain furnace, or, what is preferable, a potter's furnace is afterwards used. The more tranquil and prolonged that the fusion is, the more hardness and beauty does the strass acquire.

Strass.—The following three mixtures give a very fine strass:

Rock Crystal, -	0.318	0.3170	0.300
Minium, [illegible]	0.490	0.4855	0.565
Potash, [illegible]	0.170	0.1770	0.105
Borax, [illegible]	0.021	0.0200	0.030
Arsenic, [illegible]	.001	0.0005	
	000	1.0000	1.000

M. Lançon recommends the following mixture for a pure strass:

Litharge,	- -	0.540
White Lead,	- -	0.406
White Tartar, or Potash,		0.054

Topaz.—The imitation of topaz is difficult. It passes from the white of strass to sulphur-yellow, violet and red purple, according to circumstances which are not determined. The following are two of M. Wieland's recipes:

White Strass,	- -	0.95816	0.99
Glass of Antimony,	- -	0.04089	
Purple of Cassius,	-	0.00095	
Oxide of Iron,	-		0.01
		1.00000	1.00

These mixtures sometimes yield an opaque mass, translucent at the edges, and of a red colour in thin plates. By mixing it with eight times its weight of strass, and keeping the mixture in fusion for thirty hours in a potter's furnace, the result is a fine yellowish crystal. This crystal re-melted by the blowpipe, produces the finest imitation of eastern *Ruby*.

Ruby.—A ruby less beautiful, and of a different tint, may be made thus:

Strass,	-	0.9755
Oxide of Manganese,		0.0245
		1.0000

Emerald.—This paste is very easily made; and that which approaches the nearest to the mineral is the following:

Strass,	-	0.98743
Green Oxide of Copper,		0.01200
Oxide of Chrome,	-	0.00057
		1.00000

The following is M. Lançon's recipe for emerald:

Strass,	-	0.9905
Acetate of Copper,		0.0080
Peroxide of Iron,	-	0.0016
		1.0000

Peridot.—By augmenting the proportion of oxide of chrome and oxide of copper in the first composition of emerald, and adding oxide of iron, we may vary the green shades, and imitate the peridot and deep coloured emerald.

Sapphire.—The composition for this paste is,

Strass, very white, -	0.9855
Oxide of Cobalt, very pure,	0.0145
	1.0000

This mixture must be put into a Hessian crucible, carefully luted, and remain thirty hours in the fire. If the process is well conducted, the result will be a very hard glass, without bubbles.

Amethyst.—Very deep amethyst may be obtained with

Strass, - -	0.9870
Oxide of Manganese,	0.0078
Oxide of Cobalt, -	0.0050
Purple of Cassius, -	0.0002
	1.0000

M. Lançon uses,

Strass, - -	0.9977
Oxide of Manganese,	0.0022
Oxide of Cobalt, -	0.0001
	1.0000

Beryl or *Aquamarine* is made with

Strass, - -	0.9926
Glass of Antimony,	0.0070
Oxide of Cobalt, -	0.0004
	1.0000

Syrian Garnet.—This paste is used for small jewels, and is made with

Strass, - -	0.6630
Glass of Antimony,	0.3320
Purple of Cassius, -	0.0025
Oxide of Manganese,	0.0025
	1.0000

In the fabrication of the pastes, many precautions are necessary, which can be learned only by experience. The materials should in general be carefully pulverised. The mixtures should be properly sifted, and the same sieve should not be used for different compositions. In order to obtain the glass well melted, and homoger [illegible] nd without striæ and bubbles, materials of great purity [illegible] ved;—they must be mixed in a state of extreme t[illegible] st crucibles must be used;—the fire must be graduated, and kept equal to the maximum temperature,—and the mass must be left in the fire from 24 to 30 hours, and allowed to cool very slowly.—See *Bulletin de la Societé d'Encouragement*, tom. xviii. p. 311.

6. *Description of a New Blowing-Machine.*

This blowing-machine, which is used in the Hartz, is represented in Plate V. Fig. 5. It consists of a chain of floats moving round a wheel A. These float-boards are impelled by the weight of the current of water MN; and it is manifest, that, for a given velocity of the machine, the quantity of water admitted upon the floats may be such, that the space between two consecutive floats shall not be filled with water. The rest of the space will therefore be filled with air, which will be conveyed into the reservoir R, and will escape at S, or may be conducted wherever we please. The theory of this machine has been investigated by M. Clapeyron. The great simplicity of this blowing-machine may render it very useful, under particular circumstances; but it is obvious that the friction must be considerable.—See the *Annales des Mines*, tom. vii. p. 3.

7. *Account of Improvements made on Steam-Packets, and other Vessels.* By David Gordon, Esq.

Mr Gordon, to whom the public is indebted for the portable gas-lamp, and other inventions, has taken out a patent for different improvements on Steam-Packets.

Fig. 6. of Plate V. represents an horizontal plan of the deck of a steam-boat or steam-packet, adapted to sea or river navigation, constructed according to Mr Gordon's improvements; and Fig. 7. a longitudinal section of the same. A represents the paddle-wheel, which may be constructed in the manner of ordinary steam-boats; but it is inclosed in a case in such a way that the sides or edges of the paddles have just liberty to move freely between the sides of the case; the wheel is inclosed by the case in front (or towards the bow of the vessel), leaving only an aperture at *a* for the water to arrive at the paddles of the wheel; and this aperture should be entirely below the level of the water-line. The space directly under the paddle-wheel at *b b*, is also closed by a boarding or otherwise, at such a distance as exactly to clear the circumference of the paddle-wheel, at the same time that the hindermost part of the paddle-wheel (or towards the stern of the vessel) is left as open and free from obstruction as possible, as will be clearly seen by the section Fig. 7. By this arrangement and manner of inclosing the paddle-wheels, the water cannot escape laterally from the action of the paddles, neither can it escape downwards, but is obliged to enter at the aperture *a*, and be fairly driven out behind the paddle-wheel, where it will meet with no material obstruction; by which means the greatest possible effect is produced by the paddle-wheels, to propel the vessel forwards through the water; at the same time the paddle-wheels would always be protected from being injured by the violence of the waves, and would be less impeded by back-water.

The second of Mr Gordon's improvements is applicable to sailing vessels as well as to steam-packets, being for the purpose of keeping a vessel perpendicular in the water (or upon an even keel, as it is termed by seamen). It consists in employing one or more vessels or tanks, to be suspended over the weather-side of the ship at a considerable distance from the side. These vessels or tanks should be suspended, in such a manner as to be capable of being lowered down into the sea, filled with water, and then drawn up to operate by their weight in counteracting the effect of the wind upon the vessel, and keeping her always upon an even keel, or nearly so, which is of very great importance, particularly in vessels or packets to be propelled by steam, in order that the paddle-wheels may act equally in the water on both sides of the vessel. The vessels or tanks above mentioned may be made of wrought iron-plate, or of tarred canvas distended by a hoop, or any other suitable material, and be furnished with a guy-rope at bottom, for the purpose of turning out the water to empty the vessels or tanks, when the use of the weight becomes no longer necessary: they may be suspended from the ends [redacted] ooms or davits over the ship's side, or from turning jibs rane, so as to be capable of being turned in clo de of the ship, when not in use.

The third of Mr Gordon's improvements, which is applicable to steam-packets and other vessels, is to surround the ship or vessel at about the height of the gunwale with a chevaux-de-frise or line of railing, which is to be very thickly set with pikes (like a hedge-hog's back); and to prevent any accidents, from objects falling upon the sharp points, iron rods and an iron-netting might be placed round the chevaux-de-frise. This chevaux-de-frise or assemblage of pikes might be made in a number of separate pieces, so as to be bolted, or otherwise fixed, on the outside of the ship when it goes to sea. By this means no serious injury would be done by large waves coming on board a vessel, or the ship would not be in danger of being pooped, although no dead-lights were employed: at the same time, it would be a sufficient protection to cabin-windows, for it is only when water is kept in a compact body that it is so very powerful; the instant that its particles are separated and intermixed with air, which is a compressible substance, its great power is destroyed.

8. *Notice respecting the Escapement of M. Jurgensen.*

In our last Number, p. 148. we have given an account of the escapement proposed by M. Jurgensen, but without any engraving of it, as the author had not annexed any to his first paper. M. Jurgensen has, however, given an engraving of his escapement in a subsequent paper, which we have copied in Plate V. Fig. 8. and 9.

Fig. 8. is a plan, and Fig. 9. a profile view of it, where *a* is the impulse-wheel, *c* the circle of the escapement, and, *b* the wheel which produces the pause, *eer* the detent-spring, pressing by its elasticity against the fine point of the screw *m*, and carrying a pallet *n*, upon which the teeth of the wheel *b* rest during the vibrations. The spring *go* is in the same plane as the circle or roller *vs*, which carries a ruby pallet *s*.—See *Shumacher's Astronomische Nachrichten*, No. 14. p. 210.

Art. XXVI.—*Geological Thermometer, shewing the Opinions attri- buted to various Geologists with respect to the Origin of Rocks.*

Whiston, Theory of the Earth, 1725. **Buffon**, Theorie de la Terre. **Leibnitz**, Protogæa, 1768. **Descartes**.	100 95	*Plutonic Region.*	All Rocks affected b heat. The Earth stru off from the Sun by Comet.
Boué, Essai sur l'Ecosse, 1822.	90		All Rocks of Chemic origin igneous.
Hutton, Theory of the Earth, (Ed. Trans. v. i.) **Playfair**. Illustrations, 1820. Sir J. **Hall**, Edin. Trans. vol. vi. 1806. Sir G. **Mackenzie**, Travels in Iceland, 1810.	85		All the older rocks eith fused or softened b heat. Metallic Vei injected from below.
Sir H. **Davy**, On Cavities in Rock Crystal, 1822. **MacCulloch**, Various papers in Geol. Trans. from 1814 to 1817. **Knight**, Theory of the Earth, 1820. **Breislac**, Journal de Physique, vol. xciii.	80 75		Granitic Rocks igneo Granitic Veins injec ed from below. Some Granite and Sy nites igneous.
Faujas St Fond, Essais Geologiques. **Humboldt**, Travels and Memoirs. **Spallanzani**, Sur les Isles Ponces. Sir W. **Hamilton**, Memoirs, &c. **Dolomieu**, Voyage aux Isles de Lipare, 1783. **Saussure**, Voyages dans les Alpes, 1787. W. **Watson**, Section of Derbyshire, &c. **Whitehurst**, Theory of the Earth, 1786.	70 65 60	*Volcanic Region.*	All Trap Rocks igneous
Cordier, Sur les Substances Minerales dites en masse, 1815. **Von Buch**, Travels, Memoirs, &c.	55		Augite Rocks igneous.
Buckland, Memoirs. **Conybeare**, Geology of England, 1822. **Sedgwick**. **Henslow**.	50 45		Flœtz-trap Rocks ign ous. Whin-dikes in jected in a fluid st from below.
Dolomieu, Journal de Phys. vol. xxxvii. 1790. **Saussure**, Journal de Phys. (an. 2.) 1794. **Daubuisson**, Ib. 1804, Sur Volcans d'Auvergne. **Daubeny**, Edin. Philos. Journ. 1821, On the Volcanoes of Auvergne.	40 35		Some Flœtz-traps ign ous; others aqueous
Daubuisson, on the Basalts of Saxony, 1803. **Deluc**, Treatise on Geology, 1809. **Klaproth**, Beiträge, vol. iii. **Jameson**, Edinburgh Philos. Journal, 1819. **Richardson**, On the Giant's Causeway, Ph. Tr.	30 25	*Neptunian Region.*	Igneous origin of a Trap Rocks questio ed. Whin-dikes co temporaneous with th rocks they traverse.
Macknight, Wernerian Memoirs, 1811. **Jameson**, Geognosy, 1808. **Murray**, Comparative View, 1802. **Mohs**, Memoirs, &c. **Kirwan**, Geological [illegible] 1795. **Walker**, Lectures, [illegible] **Werner**, Theory of [illegible]	20 15 10		All Rocks (except th Volcanic) deposite from aqueous solutio Metallic Veins pou ed in from above.
Lamarck, Hydrogeo[illegible] **Demaillet**, Telliam[illegible]	5		Secondary Rocks secre ed by animals and ve getables, and forme out of Water.

December.

D	H	′	″		D	H	′	″	
1.	2	35	25	Im. I. sat. ♃	17.	0	53	55	Em. I. sat. ♃
	18	12	15	☌ ☽ β ♊	18.	19	22	42	Em. I. sat. ♃
2.	19	51	40	☌ ☽ δ ♋ *	19.	8	39	23	☌ ☽ ⊕ ♒
	21	4	5	Em. II. sat. ♃	21.	14	2	5	☽ First Quarter.
3.	0	40		☌ ♂ ♅, ♂ 45′ S. of ♅		17	15	18	Im. III. sat. ♃
4.	8	14	10	☌ ☽ α ♌		19	32	57	Emersion — —
5.	0	15	26	Em. II. sat. ♃	22.	8	5	46	☉ enters ♑
	12	24	14	☽ Last Quarter.		18	48	4	Em. II. sat. ♃
7.	0	48	33	☌ ☽ η ♍	23.	18	8		Sup. ☌ ☉ ☿
	14	21		☌ ☿ β ♏, ☿ 47′ S. of *	24.	1	38		☌ ☽ ♄
8.	4	30	12	Em. 1. sat. ♃		2	49	4	Em. I. sat. ♃
9.	22	58	54	Em. I. sat. ♃	25.	5	5	55	☌ ☽ η ♉ *
10.	0	49	54	☌ ☽ α ♎		16	48		☌ ☽ ♃
11.	17	27	40	Em. I. sat. ♃	27.	4	50	35	☌ ☽ β ♉
	18	47	8	☌ ☽ δ ♏	28.	5	52	58	○ Full Moon.
	19	29	43	☌ ☽ π ♏		8	10	25	☌ ☽ ε ♊ *
	19	59	54	☌ ☽ β ♏		11	36		☌ ♀ ♅, ♀ 17′ S. of ♅
12.	2	52	38	Em. II. sat. ♃		21	16	9	Im. III. sat. ♃
	10	3		☌ ☽ ☿	29.	15	54		☌ ☉ ♅
13.	7	35		☌ ☽ ♀		21	25	9	Em. II. sat. ♃
	13	11	14	● New Moon.	30.	6	12	50	☌ ☽ δ ♋ *
14.	20	54		☌ ☽ ♅	31.	9	30		☌ ☿ ♅, ☿ 82′ S. of ♅
15.	14	58		☌ ☽ ♂					

The times inserted in the preceding list are given according to the French method, reckoning the day to commence at midnight. It appears by the preface to the Nautical Almanack for 1813, that Dr Maskelyne, by introducing the French Tables in the calculations, had intended to adopt this method, although, on farther consideration, he thought proper to relinquish it.

I have calculated the eclipses of Jupiter's satellites which are marked visible in the Nautical Almanack, from the New Tables of M. De Lambre, printed in 1817. In reducing these Tables to the meridian of Edinburgh, I have allowed 9′ 21″ for the Long. of Greenwich W. of Paris, and 12′ 41″ for the Long. of Edinburgh W. of Greenwich.

The true ecliptic conjunctions of the Moon with the Fixed Stars, will be found sooner or later than the apparent time, according as the moon's parallax in longitude increases or decreases the moon's [illegible] The conjunctions of the moon with the stars mark [illegible] risk, are those which will most likely produce oc [illegible] I have not had leisure to calculate the times o [illegible] na.

Art. XXVIII.—*Proceedings of the Royal Society of Edinburgh.* (Continued from p. 165.)

June 17. A Paper by the Reverend Dr Fleming of Flisk was read, entitled, " On a Submarine Forest in the Frith of Tay, with Observations on the Formation of Submarine Forests in general."

This very interesting geological phenomenon is similar to the one observed on the coast of Lincolnshire, and described by Dr Joseph Correa de Serra in the *Philosophical Transactions* for 1799. It occurs on the south bank of the Frith of Tay, and has been observed in detached portions on each side of Flisk Beach, to the extent of nearly three miles. After explaining the general and particular appearance of this submarine forest, Dr Fleming proposes a very ingenious and rational explanation of the way in which it has been formed, and he considers his explanation as equally applicable to the submarine forest of Mount Bay, Lincolnshire, and that on the west coast of Orkney described by Mr Watt of Skail in this *Journal*, Vol. III. p. 101.

The Royal Society adjourned till the 4th of November 1822.

Art. XXIX.—*Proceedings of the Wernerian Natural History Society.* (Continued from p. 167.)

1822, *May* 4.—THE Secretary read a communication from Lawrence Edmondston, Esq. of Shetland, on the Larus parasiticus or Arctic Gull. (This paper has already appeared in this *Journal*, p. 91. *et seq.* of this volume.) Mr John Stark then communicated some curious remarks regarding the habits of the Talpa Europæa or mole; and Dr Grierson of Cockpen exhibited the model of a machine for extracting roots of trees, or for similar purposes, invented by Mr John Fletcher of Bonnyrig, near Laswade.

At the same meeting, the Reverend George Young of Whitby being present, read an account of different caverns in Yorkshire, recently discovered, which have been found to contain, like those of Kirkdale, fossil remains of quadrupeds now extinct in this island.

May 18.—Professor Jameson communicated to the Society the substance of Captain Vetch's geognostical account of the Island of Foula, illustrating it by sketches. He also read a notice relative to the cannibalism of the Battas in the interior of Sumatra, by a naturalist now engaged in exploring the Indian Islands, at the instance of the British Government; together with an account of the Dryobalanops or Sumatran camphor-tree. (Already published in this volume of the *Edinburgh Philosophical Journal*, p. 87, *et seq.*) The Professor likewise read extracts from Dr Daubeny's paper on the methods of separating magnesia from lime, and on its distribution through rocks of the transition and secondary class. (Printed in the present volume of this Journal, p. 108, &c.) Mr Deuchar, lecturer on chemistry, then read an account of several curious circumstances connected with the ductility of glass, shewing that the most attenuated threads retain the character and shape, twisted, angular or tubular, of the mass from which it is spun, illustrating his remarks by an experiment, proving the passage of quicksilver through the most minute threads. The Secretary communicated Mr Lawrence Edmondston's account of the Greenland Kittiwake and Colymbus Grylle (which has already appeared in this Journal, *supra*, p. 94, &c.)

Aug. 10.—This was an extraordinary meeting, called for the purpose of addressing the King, on occasion of his visit to Scotland. An address having been accordingly agreed on, it was presented to his Majesty by Professor Jameson, as President of the Society, at the levee held at Holyroodhouse on Saturday the 17th August, and afterwards published in the Edinburgh Gazette of the 23d of that month. At the same time, an elegantly bound set of the Society's Memoirs was presented to his Majesty, and graciously received.

ART. XXX.—SCIENTIFIC INTELLIGENCE.

RAL PHILOSOPHY.

RONOMY.

1. *On the* .—A very valuable and ingenious memoir has hed in the *Irish Transactions* by

Dr Brinkley of Dublin, entitled, "On the Quantity of Solar Nutation, as affecting the North Polar Distances of the Fixed Stars, deduced from Observation, and the Application of this determination to confirm the conclusions relative to the Parallaxes of certain Fixed Stars."

Those who have examined with care the very admirable papers which Dr Brinkley has published * on the Parallax of certain Fixed Stars, can scarcely fail to be convinced, that the great discovery of the parallax has been made by that able astronomer; and we are proud to think that this has been accomplished by an English astronomer, and by English instruments. If any doubts remained upon this subject, the present memoir of Dr Brinkley must, we think, entirely remove them. It occurred to him that the great number of observations which he had made upon particular stars, might be employed to determine the *solar nutation*, which had been deduced only from theory, and had never been ascertained by actual observation. According to theory, the solar nutation is about 0″.51 in north polar distance for all stars; and as this quantity is less than the parallax of certain stars, Dr Brinkley considered, that if it were deducible from his observations, such a deduction would not only prove the exactness of his observations, but the power of his circle to determine such minute quantities. As the solar nutation goes through all its states twice every year, Dr Brinkley considers it impossible to suppose, that if any cause should occasion the instrument to shew deviations explained by parallax which did not actually exist, it should not derange the solar nutation, and cause the result of an investigation of its quantity to turn out erroneous.

By this method of investigation, applied to several stars, Dr Brinkley has obtained the most satisfactory results; and we agree with him in thinking, that there cannot remain the smallest doubt with any one who examines the processes, that the observations have ascertained with considerable exactness the quantities of parallax of α *Lyræ*, α *Cygni* and *Arcturus*;—that the parallaxes of γ *Draconis*, and η *Ursæ Majoris* are extremely small;—and that γ *Draconis* is at least *seven* or *eight* times more distant than α Lyræ. The following are the results obtained by Dr Brinkley:

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* See *Irish Transactions*, vol. xii.; and *Philosophical Transactions*, 1818, p. 275, and 1821, p. 327.

	Maximum Solar Nutation.	Parallax.
α Lyræ, -	+ 0″.5055	2″.276
γ Draconis,	+ 0 .4246	0 .141
η Urs. Maj.	+ 0 .5782	0 .190
α Cygni,	+ 0 .5572	1 .000
Arcturus,	+ 0 .4430	1 .305

2. *Dr Brinkley's Elements of the Comet of* 1821.—This interesting comet, which had been observed in Europe (see this volume, p. 174.), before it passed its perihelion, was fortunately observed by Captain Basil Hall, Lieutenant Robertson, and Mr Forster of H. M. S. Conway, at Valparaiso, after it had passed its perihelion.

That eminent astronomer, the Reverend Dr Brinkley of Dublin, to whom Captain Hall's observations were communicated, found that the computation of its elements was involved in difficulties not often experienced, in consequence of the observations having been made after its passage of the perihelion, and in consequence of the smallness of the perihelion distance, and some other circumstances. He had therefore recourse to an improved method of computation, which he has explained in the *Philosophical Transactions* for 1822, Part I. p. 53.

By this method he obtained the following elements:

Passage of Perihelion. Mean Time at Greenwich, March 21.	$13^{h}\ 15'\ 47''$
Perihelion distance, - - -	.092800
Inclination of orbit, - -	$73^{\circ}\ 15'\ 48''$
Place of ascending node, - -	$1^{s}\ 18\ 24\ 41$
Place of perihelion, - - -	7 29 6 47
Motion retrograde.	

After these elements were calculated, Dr Brinkley obtained the following new elements, by using Dr Olber's observations of January 30. and those of Captain Hall of April 8. and May 3.

Passage of perihelion, March 21. - - - -	$11^{h}\ 11'\ 48''$
Perihelion distance, - - - -	.091677
Inclination of orbit, - - - -	$73^{h}\ 34'\ 53''$
Place of nod [illegible] - - - - -	$48^{\circ}\ 42'\ 18''$
Place of pe [illegible] - - -	$7^{s}\ 29\ 30\ 33$

Between [illegible] d May 3. the comet described above 800° ab [illegible] d as a parabola represents the observations with so much exactness, Dr Brinkley concludes that the period of its revolution must be considerable.

This comet is remarkable on account of its small perihelion distance. In its great inclination, and its small perihelion distance, it agrees with the comet of 1593.

On the 2d April, Captain Hall observed that the tail of the comet seemed to be split, or to have a dark streak between its sides. On its first appearance it was of a dull-white colour.

3. *Length of the Pendulum at Madras.*—A series of very accurate observations has been made at Madras by John Goldingham, Esq. F. R. S., for determining the length of the pendulum in latitude 13° 4′ 9″.1. The pendulum employed was one on Captain Kater's principle, and made under the direction of that able astronomer.

By the first series of observations, made at Madras, the length of the seconds' pendulum was 39.026323087, the number of vibrations in 24 hours being 86172,375. By the second series its length was 39.026280447, the number of vibrations in 24 hours being 86172,328.

The mean of both these results is 39.026302 inches, which, according to Sir George Shuckburgh's scale, is the true length of the seconds' pendulum at Madras, in latitude 13° 4′ 9″ N., at the level of the sea in vacuo, and at a temperature of 70° of Fahrenheit.

Captain Kater found that the same pendulum made 86293,14 vibrations in a mean solar day, in latitude 51° 31′ 8″.4, the temperature being 70°, and the height above the level of the sea 83 feet.

By comparing the mean length at Madras with 39,142213 inches, the length in Lat. 51° 31′ 8″.4 N., the diminution of gravity from the Pole to the Equator will be 10052897, and the ellipticity of the earth $\frac{1}{297.3}$ nearly. See the *Phil. Trans.* 1822, Part I. p. 167. &c.

4. *Derangement of the great Mural Circle at Greenwich.*—Mr Pond has lately discovered that a derangement has taken place in the mural circle at the Royal Observatory. Although the telescope may be applied to every part of the mural circle, yet, when fixed for observations, the principle of the instrument re-

quires that the tube, especially at its extremities, should be so firmly fixed to the circle as to form one piece with it. In order to do this, connecting braces are attached at each end of the telescope. In progress of time, however, these braces have become insecure, owing to the screws which fastened them having given way. Hence the ends of the telescope are permitted to bend from the centre, instead of retaining an invariable position with respect to the circle. In the zenith this error is a minimum, and increases from the zenith to the horizon. This evil has now been remedied by Mr Troughton, who has applied additional braces to connect the telescope with the circle. This alteration has already produced a great improvement in the observations. Of the published observations, only those made in the three last months of 1819 are affected by the error. During 1820 the error increased, but, in the distance from the Pole to the Equator, did not amount to 2 seconds, at altitudes lower than Sirius, and at the altitude of the sun at the winter solstice, the error may have exceeded 2 seconds, but did not exceed 4 seconds. After the month of February 1821, the error rapidly increased, which ultimately led to the discovery of its cause. —See the *Phil. Trans.* 1822, Part I. p. 86, &c.

5. *Curious appearance observed on the Moon at the Cape.*—The Reverend Fearon Fallows, the astronomer at the new observatory erected at the Cape of Good Hope, observed with his naked eye a whitish spot on the dark part of the moon's limb. This happened on the 28th November 1821, about 8 o'clock P. M., when the moon was shining with a brilliancy which he had never observed in England. This spot seemed now and then to flash with considerable lustre. With an achromatic telescope about 4 feet long, magnifying 100 times, the spot seemed like a star of the 6th magnitude, and there were *three others* much smaller, but *one* of these was more brilliant than the one he had seen with the naked eye. The largest spot was surrounded by a nebulous appearance, but nothing of this kind was observed about the small brilliant spot. The two others were similar to faint nebulæ, increas intensity towards the middle, but without any defined lu As the telescope with micrometers had not yet a ape Observatory, Mr Fallows had

not the means of determining the exact position of these spots. On the evening of the 29th November, the large spot was fully as bright as before, two others were nearly invisible, and the small brilliant spot had disappeared.—See *Phil. Trans.* 1822, Part I. p. 237, 238.

6. *Comet of* 1822.—M. Gambart first discovered this comet at Marseilles, on the 12th May 1822. It was observed at Paris on the 18th May, and from these observations M. Nicollet has computed the following elements of its orbit.

Passage of the perihelion 6th May 1822, at	3^h 5′ 11″ A. M.
Perihelion distance,	0.504220
Inclination of its orbit,	53° 34′ 3″
Longitude of ascending node,	177 30 50
Longitude of perihelion on orbit,	192 48 45

Heliocentric motion retrograde.

This comet is very small, and has no appearance of a tail. It has no resemblance in its elements to the comet that is expected, nor to any other.

7. *Account of Rieussec's Chronograph for noting small intervals of Time.*—This instrument has the form and size of a large pocket chronometer. The dial-plate is moveable on an axis passing through its centre. This dial makes a revolution in a minute, and each of its divisions corresponds to a second of time. When the observer wishes to mark the precise instant when any phenomenon takes place, he presses a button, and a small pin or metallic point, traversing the summit of a cone filled with printers' ink, and placed opposite the fixed zero of the moveable dial, marks on the circumference, divided into seconds, a very fine point, which indicates the second, or fraction of a second, which corresponds to the beginning and end of the interval of time which it is wished to measure. The instantaneous contact of the point has no influence on the motion of the dial, and the fineness of the point enables us to estimate the fourth part of one of the divisions of the dial-plate.—*Bib. Univers.* June 1822, p. 158.

METEOROLOGY.

8. *Depression of the Barometer in England, on the 25th December* 1821.—Mr Luke Howard made the following observations on the barometer on the 24th and 25th December 1821, at Tottenham Green:

		Inches.	Temperature.
December 24.	8^h	28.20	45°
	11	27.96	
December 25.	5 A. M.	27.83	

which was probably the minimum. In March 1783 it stood at 27.88 during the Calabrian earthquakes. The barometer of the Royal Society's apartments in Somerset House, at one foot above low-water of spring-tides in the Thames, stood at 28.18 at 8 o'clock in the evening of the 25th December.—See the *Phil. Trans.* 1822, Part I. p. 113.

9. *Observations on the Barometer and Thermometer at Berlin, 25th December* 1821.

The Barometer was placed about 40 feet above the river.

		Barom. In. Lines.	Therm. attached.	Temp. of the Air.
1821, Dec. 25.	$0^h\ 30^m$ A. M.	27.1,08	12°.5	2°.2
	9 25 A. M.	26.9.39	11 .5	5 .9
	1 0 P. M.	26.8.74	12 .5	
	2 45 P. M.	26.8.94	12 .5	6 .8
	10 0 P. M.	26.10.6	13	6 .3
26.	1 10 A. M.	27. 0.0	13	4 .5

Inches.
The mean height of the barometer at Berlin is 28 0 3

The scale of the barometer is divided into inches and 12th parts ancient French measure. The scale of the thermometer is Reaumur's.—*Communicated* by Professor Tralles of Berlin.

10. *Singular contrast between the Winter of* 1821–2 *in the North of Europe and South America.*—No fact in meteorology is more remarkable than the mildness of last winter in the North of Europe and Asia, and its severity in South America. The winter at St Petersburg, which is always very severe during four successive months, and continues two months more, so as to last *six* months, lasted only about a month and a few days. The first snow fell at Christmas, and it disappeared generally on the 1st day of February. Although the weather in the interior of Russia was colder and more constant than at St Petersburg, yet there was no snow, and the frost did not commence till the [illegible] of January. On the 2d March the Dwina was free of i[illegible] The winter in Siberia was equally mild. Warn[illegible] d both at Tobolsk and also farther to the north-e[illegible] re there was no snow. At Bere-

soff, one of the most northern of those districts, it rained abundantly on the 8th December, a circumstance which had never occurred in the memory of the oldest inhabitants.

Near Buenos Ayres there fell in the month of December 1821 such a quantity of snow, that the communication between that city and Lima was entirely interrupted. The cold which prevailed in the different parts of South America is a most extraordinary phenomenon, and the inhabitants of Peru and Chili have considered it as a true calamity.—*Bibl. Universelle*, June 1822, p. 108.

MAGNETISM.

11. *Dip of the Needle at London in* 1821.—From a series of numerous and accurate experiments made by Captain Edward Sabine, he obtained the following measures of the dip of the magnetic needle at London, in the Regent's Park.

By ten experiments with Tobias Mayers'[*] needle, -	70°. 2′.9
By the times of oscillation in the magnetic meridian, and in the plain perpendicular to it. Mean by *three* needles, - - - - - -	70. 4
By the times of vertical and horizontal oscillation, -	70. 2.6
Mean,	70. 3.2

Hence Captain Sabine concludes, that 70° 3′ is the mean dip of the needle at London, in August and September 1821, within four hours of noon.

As the observations of Nairne and Cavendish give 72° 25′ for the dip in 1774, we obtain 3′.02 as the mean annual rate of diminution between 1774 and 1821.

Taking Mr Whiston's determination of the dip in 1720, viz. 75° 10′, which Mr Cavendish considered as accurate, the annual diminution is 3′.05.

II. CHEMISTRY.

12. *Nitrogen Springs in Rensselaer County, New-York.*—The most remarkable springs in the county of Rensselaer, are the nitrogen springs in the south-east corner of the town of Hosick. They are about six miles south-west of the village of Bennington, in Vermont. There are three springs comprised

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[*] *Mem. R. S. Gotting.* 1814.

within about four or five acres of ground. The quantity of *pure nitrogen gas*, or azotic gas, which issues in the form of bubbles from these springs, is incalculable. The gas does not seem to be combined with the water, but to issue from the gravel-beds beneath the water of the springs: and we have no good reasons for saying that the gas is confined to the beds of these springs. It may continually issue from the dry parts of the soil in their immediate vicinity, and become manifest only where the water covers the soil, by bubbling through it. By pressing upon a surface of the gravel equal to five or six inches square, a quart of the gas may be collected in an inverted jar or bottle, in ten seconds.

By what process in nature is such an abundance of gas incessantly produced? About three-fourths of the atmosphere is nitrogen gas; and we are acquainted with no other extensive reservoir of this substance. If it is derived from the atmosphere, how is it introduced into this situation? Or, if this process of nature should be explained, how is the oxygen of the atmosphere separated from the nitrogen?—Eaton's *Geological Survey of Rensselaer County*, p. 29. Albany, 1822.

13. *Analysis of Gibbsite, a new Mineral.*—This new mineral, named after Colonel Gibbs, was discovered by Dr Ebenezer Emmons, in a neglected mine of brown hæmatitical iron-ore in the town of Richmond, in Massachusets.

It occurs in small irregular stalactitic masses, from 1 to 3 inches in length, and 1 inch, or more, in breadth, consisting of an aggregation of elongated tuberous branches, laid parallel, and united together.

It is rather harder than calcareous spar, is slightly translucent, and has a specific gravity of 2.40. It does not effervesce with acids, and whitens before the blowpipe.

According to the analysis of Dr Torrey of New-York, its composition is,

Alumine,	-	-	64.8
Water,	-	-	34.7
Loss,	-	-	1.5
			100.0

Hence this mineral is a *Hydrate of Alumine*, and is the same as what *Wavellite* was supposed to be, before Berzelius detected in it phosphoric acid. Dr Torrey was particularly careful in ascertaining that Gibbsite contained neither *Phosphoric* nor *Fluoric* acids, both of which are ingredients in Wavellite. Nearly all the Gibbsite yet found, has been collected at the mine in Richmond, where it was originally discovered.—*New-York Medical and Physical Journal* by Drs Francis, Dykeman, and Beck, No. I. p. 68.

14. *Cathartine, or the Active Principle of Senna.*—MM. Lassaigne and Fenuelle, have discovered the active principle of *Senna*, and which they have called *Cathartine*. The precipitate obtained by acetate of lead from a filtered decoction of the leaves, was diffused through water, and sulphuretted hydrogen passed through it. The liquor filtered was evaporated to dryness, and digested in alcohol, and the alcoholic solution was then evaporated to dryness. It contained acetate of potash, which was separated by alcohol acidulated by sulphuric acid. The sulphate of potash insoluble in this fluid, being separated by the filter; the excess of sulphuric acid was precipitated by acetate of lead; then, by decomposing this latter salt, by sulphuretted hydrogen, and again filtering and evaporating to dryness, the *Cathartine*, or purgative principle of senna, was obtained.

The Cathartine does not crystallize; is of a reddish-yellow colour, and has a peculiar smell, and a bitter and nauseous taste. It is insoluble in ether, but soluble in alcohol and water, in all proportions. The extract becomes moist in the air. It operates as a purgative when taken in very small doses.

15. *Analysis of the Meteoric Stone of Juvenas* *.—In analysing this meteorite, M. Vauquelin found in its crust, grains of silex, and yellow brilliant points, like sulphuret of iron. It contained no nickel, but traces of copper and potash. It contained more alumina and lime, but much less magnesia than other meteorites. The iron which it contained did not act upon the most delicate magnetic needle, and a portion of it was united to chrome. The *Silex* amounted to *two-fifths* of the total weight of the stone.

16. *Soda Alum.*—This new salt described by Dr Ure in the *Journal of Science*, has the same form and taste as common

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* See this *Journal*, vol. vi. p. 384.

alum. The specific gravity is 1.35. The following is the results of Dr Ure's analysis:

Sulphuric acid, - -	34.00
Alumina, - -	10.75
Soda, - -	6.48
Water of crystallization, -	49.00
	100.23

Dr Ure is of opinion that this salt is well adapted for certain purposes in calico-printing, by its remarkable solubility in water.

By distilling a portion of its solution off lime, he ascertained that it contains no ammonia.

17. *Melanic Acid in Black Urine.*—Dr Marcet has described, in the 12th volume of the Medico-Chirurgical Transactions, a singular variety of urine, which turned black soon after it was discharged; and he has given the following notice of its chemical properties, as communicated to him by Dr Prout:—"The residuum obtained from this urine by evaporation, not only does not contain any lithic acid, but no urea can be detected in it. Although the addition of dilute acids produced no immediate change of colour in the urine; yet, on standing for some time, a black precipitate slowly subsided, leaving the supernatant fluid transparent, and but slightly coloured. The black precipitate thus obtained, was found to be nearly insoluble either in water or alcohol, whether hot or cold It readily dissolved in cold concentrated sulphuric and nitric acid, forming a deep brownish-black solution; but, on diluting the acids with water, the black substance appeared to be again precipitated unaltered. These acids, however, by the assistance of heat, apparently decomposed it. The black substance readily dissolved in the fixed alkalies, and in the alkaline sub-carbonates, forming very dark solutions. The addition of water did not affect these solutions; but acids reprecipitated the substance apparently unchanged. When ammonia was employed as the solvent, and the excess expelled by evaporation to dryness, a black or deep brown residuum was obtained, which appeared to be a compound of the black substance with ammonia, and possessed the following properties. It was very solub[illegible]ter; and, on being heated with caustic potash, it gave off [illegible] ammonia. The black compound, however, did [illegible]ve any tendency to assume the crystalline form. [illegible]tions of this compound in water, mu-

riate of barytes, and nitrate of silver, produced copious brown precipitates, as did also protonitrate of mercury and nitrate of lead. But oxymuriate of mercury produced an immediate precipitate, and that obtained from acetate of zinc was of a paler brown colour." From these experiments, Dr Prout considers the black principle obtained from the urine by the action of dilute acids, as a new body, possessed of acid properties; and, if this is confirmed by farther observations, he proposes to call it *Melanic Acid.*

18. *Conversion of Iron into Plumbago.*—The following interesting fact has been pointed out to us by a correspondent, in Beckmann's *History of Inventions*, vol. iv. p. 243.—"Those who consider steel only as the most perfect kind of iron, may perhaps think with Plutarch, that the softer and more equable part of the metal is first converted into rust, and that the better part of the whole mass is thus separated from the rest. Without entering on a farther discussion of the subject, I shall only mention an observation made a few years ago. In digging up the floor of a cellar in an old house near Gottingen, there was found an anvil which had lain under the damp earth for many years. It still retained its original form and size, but was become so soft that it could be easily crumbled to pieces. When broken, there were seen, every where on the fracture, exceedingly white grains, with a metallic brightness which resembled polished steel, and were attracted by the magnet; consequently, had not yet been converted into rust."

III. NATURAL HISTORY.

MINERALOGY.

19. *The Secondary or Flœtz Formations, as determined by Werner, the same in Great Britain, as in Germany.*—Mr Weaver, a pupil of Werner, in his valuable view of the flœtz rocks in the Annals of Philosophy, concludes his details with the following remarks:

"It follows, from the whole of these premises, that the flœtz formations of Werner strictly commence with the old red sandstone of England, and not, as has been stated, with the new or calcareous conglomerate. It follows, also, that the charge of confusion in the views of that naturalist is obviated, and that so far from the flœtz formations which came under his consideration having been few in number, they comprehended the whole series, from the old red sandstone up to the chalk, and above the chalk, gravel, sand, clay, wood-coal, and the newest flœtz-trap formation. His arrangement of formations in Germany is, when duly construed, quite in accordance with their succession in the British Isles. There is no hiatus. We travel from the primary to the transition, and thence through the whole series of the flœtz, in which last, let it be observed, that, though the carboniferous series be less fully displayed, yet other formations are in much greater force in Germany, and afford a greater variety of character than is to be found in the British Isles; and here we may perceive the compensating power of nature.

"I have, therefore, yet to learn that more modern inquiries have at all invalidated the general positions of Werner. His grand outlines of the structure of the globe remain unshaken, from the fundamental granite, up to the newest flœtz-trap. The labours of his followers, and of other geologists pursuing a similar path, have tended more and more to fill up those outlines.

"The Comparative View of flœtz formations which I submitted to the public in the *Annals of Philosophy*, Oct. 1821, is consistent with the main positions of Werner, though, from the mode of considering them, there may seem to be some difference: this, however, is rather apparent than real. It arises from the following circumstances: 1. In the carboniferous series, producing the limestone and the coal as distinct formations, while Werner considered them only as members of his first flœtz sandstone, or rothe todtliegende formation: 2. In like manner, in the gypseous and saliferous series, producing the weissliegende or calcareous conglomerate as a distinct formation; while, by Freiesleben and others, it is included in the magnesian limestone formation: 3. As a consequence of the foregoing, in considering the magnesian limestone as belonging to the second flœtz series: and 4. From distributing the flœtz formations into four principal series, founded, as I conceive, on natural distinctions; namely, on their relative position in the order of succession, their mineralogical characters, the organic remains which they respectively contain, and the mutual affinities of the formations which constitute each series or group. In this view there is no real incongruity; for, in fact, had the carboniferous limestone appeared in force in the north of Germany, it certainly would have been designated by Werner as the first flœtz limestone; and this, according to the established method of that naturalist, who, in arranging the mineral masses of the globe, was led to distribute the predominant into *principal* formations, and the incidental into *subordinate*. Bearing this in mind, the carboniferous limestone would have been his first flœtz limestone formation; and, as a necessary consequence, the magnesian limestone would have become his second flœtz limestone. The whole difference, therefore, is a mere question of enumeration.

"In conclusion, I must observe, that, in awarding the meed of praise due to the services of Werner, French writers appear in general to have been more just than the English. Not a few of the latter seem to forget, or not to consider, that, though others might, before his time, have hit upon the general division of rocks into primary and secondary, yet geology, as a science, had no existence. To Werner belongs, in the first place, the merit of introducing a nicer discrimination in the examination of simple minerals, and of inventing an appropriate language by which they might be described and distinguished, previous to which mineralogical science was quite in its infancy. And, in the second place, to him also belongs the chief merit, not merely of distinguishing and giving names to rocks, but of accurately marking out both the grand distinctions of primary, transition, and flœtz classes, and the various principal formations of which those classes consist. If, then, it be the glory of the Saxon to have laid the broad foundations of the edifice, let that of the Briton and Frank be to complete the structure."

20. *Sulphate of Lead.*—Connected with an argentiferous galena, in Mr Lamb's mine at Huntingdon, is an incrustation of sulphate of lead. It is tolerably abundant, but is nowhere in crystals or in masses; it occupies merely the surface and cavities of the other ores of lead. It gives metallic lead instantly by the blowpipe, but does not effervesce nor dissolve in acids. According to Mr Lane, it is equally rich in silver as in galena. This, if correct, is we believe a new fact, but we have not examined the ore in this respect.—Silliman's *Journal.*

21. *Hyperstene.*—Dr Webster has discovered *hyperstene* in abundance in the vicinity of Boston.—*Id.*

22. *Plumbago from North Carolina.*—It is of a very fine quality, and appears well adapted both for crayons and pots. It occurs a few miles north of Raleigh, and exists in great quantities. It has long been used in the vicinity as a pigment. We are indebted for this information to the Honourable Judge Johnson, of the Supreme Court of the United States, and also for the most beautiful yellow-ochre, from the waters of the Oconee in Georgia, and for a handsome fine-grained greyish-white marble, well polished. This is from the waters of Broad River in South Carolina. Both the last are abundant.—*Id.*

23. *Rose Quartz of Southbury, Connecticut.*—This occurs abundantly, forming a large rock, about eighteen miles from New Haven. It is of a lively agreeable colour.—*Id.*

BOTANY.

24. *Dr Hooker's Exotic Flora.*—We are happy in being able, at length, to announce the appearance of the First Part of Dr Hooker's *Exotic Flora*, the publication of which it seems was for some months unavoidably delayed. This work is destined to include, as our readers are probably well aware, and, as the notice on the wrapper informs us, "Figures and Descriptions of such Plants, not natives of Great Britain, as are cultivated in gardens, or, in defect of these, of such as can be faithfully represented from well preserved specimens in the Herbaria. In all cases, preference will, of course, be given to such individuals as recommend themselves by their beauty, their history, their novelty, or some remarkable or little known characters in their flowers or fruits." The greatest pains, we are assured, will be taken in delineating the different parts of fructification, so as to exemplify, and render familiar to the botanical student, the generic, as well as specific character, and the Natural Order to which the plants belong; and if we may be allowed to judge from the plates in the *Flora Londinensis*, and in various c [illegible] of this author's publications, these dissections will form o [illegible] t important features in the book, so that, if a p [illegible] ally represented, which has already appeared in [illegible] ilar nature, we may yet expect to have it rend [illegible] the analyses of its flowers and fruit.

In regard to the access which Dr Hooker will have to new and interesting vegetables, we conceive that few naturalists can be more favourably circumstanced than our distinguished author, since he has, as it were, at his very door, the recently established, but well stored collection of the Royal Botanic Garden of Glasgow, which is almost daily receiving additions from all parts of the world. His friend, Dr Graham, our Botanical Professor, has liberally offered to Dr Hooker the use of whatever may deserve his attention, in the splendid establishment under his direction; and the Messrs Shepperd of the Liverpool Botanic Garden, have already communicated some invaluable plants from their fine collection, which have found a place in the first part of the *Exotic Flora*, now published. To these important sources of materials, we may add, that the Honourable the Directors of the East India Company, have, we are informed, in the most handsome manner, allowed him the use of a large portion of their inestimable collection, which includes drawings and manuscripts of East Indian plants, sufficient in themselves, to furnish an immense stock of materials to the botanical publisher.

We have barely space to give a brief notice of the contents of this first part, which, as will all the following ones, contains twenty plates (the folding ones counting as two.) *Tab.* 1. is the *Caladium Seguinum*, or *Dumb Cane*, so named from the circumstance, that its virulent juice, when applied to the tongue, causes a swelling which deprives the sufferer of the power of speech. This plant, a native of the West Indies, rises to the height of five or six feet; and, in this representation, the author has adopted an excellent plan of reducing the whole to a small, but highly finished figure, thereby conveying a correct idea of the habit of the plant, and subjoining portions which are of the natural size. The analyses of the fructification, magnified, here, as in the other plates, are highly satisfactory Tab. 2, *Rhipsalis Cassutha*, a plant hitherto unfigured. Tab. 3. and 4. are devoted to the beautiful, though well known *Neottia speciosa*, with ample dissections. Tab. 5. A new fern, *Aspidium Wallichii*, named in honour of the author's correspondent Dr Wallich, of the Calcutta Botanic Garden. Tab. 6. *Dorstenia arifolia*, A novel species, sent from Liverpool. Tab. 7. *Lycopodium dendroideum*, one of the *Club-mosses*, several of which are now successfully cultivated in our gardens. Tab. 8. *Doodia aspera*, a rare fern from New Holland. Tab. 9. A splendid new parasitic orchideous plant, native of the Delta of the Ganges, *Dendrobium Pierardi*. Tab. 10. *Ophrys lutea*, a charming species, and quite new to our gardens. Tab. 11. *Serapias Lingua*, a hitherto unfigured plant. Tab. 12. *Calypso borealis*; the American state of this delicate vegetable, which we yet suspect, notwithstanding what Dr Hooker has said to the contrary, may prove a distinct species from the European individual. Tab. 13. *Sarracenia rubra*. Tab. 14. *Berberis heterophylla*. Tab. 15. *Ageratum conyzoides*. Tab. 16. An elegant new *Pinguicula*, under the name of *P. edentula* from Savannah. Tab. 17. *Begonia humilis*.

We are informed that Part II., which will appear on the 1st of November, will include, besides other interesting plants, the truly beautiful *Begonia argyrostigma*, and a new *Osbeckia* from Nepal, the drawings of which the author made at the Botanic Garden of this city, during his late visit to Edinburgh, and which were then flowering for the first time in Britain.

Copies of this work are published, both plain and coloured; the former 8s. the later 15s. each Part. The plates are beautifully engraved by an artist of Glasgow, under Dr Hooker's immediate inspection: the colouring is executed by Mr Graves of London, in a style of superior excellence, as are all the other performances of that meritorious colourist. The paper is excellent in quality, and manufactured for the purpose; and the typography, as we may say of the entire publication, does honour to the state of the arts and of botanical science, in the kingdom of Scotland.

25. *Mr Greville's Cryptogamia of Scotland.*—A work has been recently commenced by Mr GREVILLE, on the *Cryptogamia* of Scotland, accompanied with coloured plates, in the manner of "English Botany." In this undertaking, Mr Greville intends to give figures of all the *Fungi* found growing in Scotland, and of all other native cryptogamic plants which have been discovered since the publication of the great work above alluded to, the "English Botany" of SMI[illegible]RBY; this last national work only wants the fungi to [illegible]t complete Flora ever published with plates, of an[illegible] Greville's continuation (published on the same [illegible]fore be regarded as a very desirable

and long wished for event. The Order *Fungi* has been continued down to the present time with few alterations, except by the Germans, who, although sometimes unsuccessful in their systematic arrangements, have contributed beyond all others to our knowledge of the structure and affinity of these intricate plants. Since the publication of the *Synopsis fungorum* of Persoon in 1801, LINK has formed them into several new Natural Orders, and given us the best sketch of a systematic arrangement drawn from natural affinity; the system also lately composed by NEES VON ESENBECK, possesses great merit; but they are both perplexed by too much subtlety and far-fetched analogy. Two volumes of a new system have likewise recently appeared from the pen of M. ELIAS FRIES, which is indubitably the best work hitherto published on the subject, as far as the generic and specific distinctions and descriptions extend; but the physiological remarks, the general arrangement, and many of the subordinate features of his plan, as detailed in the introduction, are abstruse in the highest degree, and, we must confess, little calculated to render the study more popular. It is the chief object of Mr Greville to render the study of this family as simple and intelligible as possible, and with this view the most accurate dissections accompany each species, and English as well as Latin characters are employed. He has also divided the fungi into several Natural Orders, viz. FUSIDOIDEÆ, *Grev.*—BYSSOIDEÆ, *Grev.*—GASTROMYCI, *Link.*—FUNGI and HYPOXYLA, *De Cand;* characters of which will be given at the conclusion of the first volume, so that the lichens will now, according to Mr Greville's system, be the sixth Natural Order *.

ZOOLOGY.

26. *Dr Fleming's Zoology.*—We have long regretted the want of a scientific, and at same time popular view of the leading facts in zoology. On the continent, it is true, there are many works of this description, but hitherto all those published in England have afforded but a limited and often incorrect view of general zoology. It is, therefore, with much pleasure and

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* We shall, in an early number, give a sketch of the history of Mycology, and of the various physiological and other relations of this very interesting class of plants.—ED.

satisfaction, that we announce to the public the appearance of a work by Dr Fleming, in two volumes octavo, entitled *Philosophy of Zoology*, which we have no hesitation in recommending to the particular notice, not only of the professed naturalist, but also to the general reader, as a successful arrangement of the facts and views of one of the most difficult and interesting branches of natural history. The first volume commences with an account of the peculiar characters of inorganic beings; the properties of the vital principle, the conditions of its existence, and the modifications to which it is subject, as the characteristics of organized beings; the distinguishing marks between vegetables and animals; and a view of the polity of nature. An enumeration is then given of the substances which enter into the composition of animals, and of the compounds of organization. The remainder of the volume is occupied with the different systems of organs, their structure and functions, viz. Cutaneous, Osseous, Muscular, Nervous, Digestive, Circulating, (including the Peculiar Secretions of Light, Electricity, and Heat,) and the Reproductive. Under the Nervous System, a comparison is instituted between the mental powers of man and the lower animals. The second volume is chiefly occupied with the classification of animals. It, however, commences with illustrations of the Duration of Animals, their Physical and Geographical Distribution, Revolutions, the methods of investigating their External and Internal Characters, with the principles of Zoological Nomenclature. Under the title Classification of Animals, there is a general view of the more obvious characters of the different classes, prefixed to those groups into which they are *subdivided.*

27. *Notice regarding the History and Distribution of the genus Psittacus or Parrot.*—The Greeks seem at first to have known only one species of parrot, which was imported from the east by one of the Captains of Alexander's Fleet. Aristotle, the father of naturalists, speaks of it as a rare bird, of which he had heard by report. The beauty of parrots, and their faculty of sp [illegible] on made them objects of high request among the luxurio [illegible] om the virtuous Cato justly reproaches for this p [illegible] nt. In his time, they kept them in cages of sil [illegible] d bought them at a price as high

as that of a slave. Till the time of Nero, however, they knew no other species, but those from India, when those who ministered to the pleasures of that extravagant and luxurious Emperor, found them in an island far up the River Nile, called *Gaganda.* The Portuguese, who first doubled the Cape of Good Hope, found the whole coasts of Africa and the Islands of the Indian Ocean peopled with various tribes of parrots, totally unknown in Europe, and in such vast numbers that it was with difficulty they could be prevented from devouring the rice and maize. These, however, were far inferior to the numbers and variety that presented themselves to the first adventurers in the New World. Some of the islands there were called the Parrot Isles, from the vast quantity of these birds that flocked upon them. They constituted the first articles of commerce between the inhabitants of the Old and New Continents. In those regions every forest swarmed with them, and the rook is not better known in Europe than was the parrot in the East and West Indies. So great is their variety, that nothing seems more remarkable than that only one species should have been known to the ancients at a period when they boasted of being masters of the whole world. Of more than two hundred species now known, scarcely one naturally breeds in the countries that acknowledged the Roman power, a striking proof how ill founded the pretensions of that people were to universal dominion. The Green Paroquet, with a red neck, is the first of this genus that was brought into Europe, and it is now only known by the descriptions given of it by the ancients. Birds of this kind are said to be subject to diseases unknown to the rest of the feathered tribe. Many of them die of the epilepsy and the gout. They have been separated into two great divisions, those of the Old and those of the New World,—the former into cockatoos, parrots, lories, and paroquets; the latter into acas or maccaws, amazons, criks, popinjays and paroquets. The lories inhabit the Moluccas, New Guinea, and other Asiatic Islands. They do not occur in America. Owing to their powerless flight, the birds of this tribe inhabiting one island of an archipelago, are often of a different species from those of a neighbouring one. The Touis, or short-tailed parrakeets, are the smallest of all the American parrots. They only equal the size of a sparrow, and are generally incapable of speech. The geographical distribution of this tribe of birds is probably more extensive than is usually supposed. A species called the Carolina parrot, Latham says, inhabits Guiana, and migrates into Carolina and Virginia in autumn; feeds on corn and kernels of fruit, particularly those of cypress and apples; comes into Carolina and Georgia in vast flights, doing great damage in orchards, by tearing in pieces the fruits for the sake of the seeds, the only part agreeable to its palate. Has been known to breed in Carolina, but the greater part retire south in breeding time, and return when the fruits are ripe. Mr Abbot says it is called in Georgia the Parrakeet. Bartram observes, that it never reaches so far north as Pennsylvania, which is singular, as it is a bird of very rapid flight, and could easily perform the journey in 10 or 12 hours from North Carolina, which abounds in all those fruits in which it delights. Another species (by some supposed to be only a variety of the preceding) called the Illinois Parrot, migrates from South America far northward, being common on the banks of the Ohio, and the southern shores of Lake Erie. They fly in flocks, and feed, among other things, on chesnuts, acorns, and wild pease. A third species, called the Emerald Parrot, is also an inhabitant of a comparatively cold climate. It is said to occur in the neighbourhood of the Straits of Magellan. Of this, however, Buffon has expressed great doubt, for he asserts, that no parrot is met with in so high a latitude. In this opinion he was perhaps guided by the general belief of their living on fruits and succulent food only, and the strength of his objection must therefore cease, when it is known that several of the genus feed on seeds and berries. Latham thinks the fact of their pretty extensive geographical distribution has been now asserted by too many authors of veracity to admit of doubt. We are told that two sorts were seen about Trinity Harbour in the South Seas, Lat. 41° 7′. Dr Forster met with two kinds at Dusky Bay, New Zealand, Lat. 46° south, and large flocks as low as Port Famine, in the Straits of Magellan, Lat. 53° 44′ south, where their food must have been buds and berries, the forests being frequently bounded by mountains covered with eternal snow. Buffon confines parrots within 25 degrees on each side of the Equator; but the preceding extracts clearly demonstrate the fallacy of such opinion. This numerous and splendid genus contains not fewer than 239 species.

28. *Luminous property of the Ocean as derived from Insects.*—The Memoirs of the Academy of Sciences (Savans Etrang. tom. iii. p. 267.), contain the observations of *Godeheu de Riville* on two minute marine animals, one of which is very remarkable for its phosphorescent qualities. It is not, strictly speaking, an insect, but belongs to that subclass of the *Crustacea*, called *Entomostraca* by the French naturalists. Latreille regards it as falling under the genus *Lynceus* of Muller, which is one of the many necessary generic branches of the Linnæan *Monoculus.*

"Among the various phenomena," says the author alluded to, "of which the causes are still almost unknown, that starry brightness, which we so often perceive in the slightly agitated waters of the ocean, greatly merits the attention of those who have a taste for physics; but I have not yet read any thing satisfactory on the subject, as writers seem to have satisfied themselves rather with conjectural reasons, than by making experiments to ascertain the real origin of this natural phosphorus."

During a voyage made by our author to the East Indies, he was enabled to make the following observations: About 9 in the evening of the 14th July 1754, being in N. Lat. 8° 47′, and E. Long. 73°, from Paris, he was informed that the sea appeared like a sheet of fire. Every portion of its surface, when gently agitated, broke into a thousand stars. Each wave which coursed along the side of the vessel, spread out a pure, shining, silvery light. The more distant swelling of the waters presented the appearance of a moving plain covered with snow, and the wake of the vessel was of a clear and luminous white, sprinkled over with brilliant spots of azure light (*azurés*).

"Anxious," he continues, "to consider a spectacle, to me alike novel and interesting, I was struck by the light shed by certain small bodies, which frequently remained attached to the helm, when the sea for a moment retired; and, without listening to all that was said regarding the supposed cause of the phenomenon, I ordered a bucket of water to be drawn up, and filtered into a basin through a fine linen handkerchief. After this operation, I observed that the filtered water was no longer luminous, but that the handkerchief was covered with many brilliant points. Some of these I raised on the end of my finger, and found that they had a certain consistence as animal bodies; being thus exposed, they gradually lost their brightness, and as they resembled the eggs or spawn of fishes, in form and dimensions, I at first yielded to the belief, which was pressed upon me, that they actually were so.

Being anxious to examine one in a clear light, and placed under a strong magnifying glass, I was surprised to observe a sensible movement in its interior. Being doubtful of what I saw, I turned it in many directions, placing it on my nail in the centre of a drop of water. But what was my surprise, when I perceived it became surrounded by a brilliant fluid, perceptible to all those who were in the cabin, as well as to myself. On this I did not fail to pursue my observations, and having drawn up a greater quantity of water, I caused it to be filtered as before, and immersed the handkerchief, which had served for that purpose, into a basin of the pure sea-water. I then instantly perceived a considerable number of small insects swimming about with celerity, which, at first sight, bore a resemblance to those commonly called in France *Puces d'eau*, or Water-fleas. In spite of their agility, I succeeded in arresting one, by entangling it in a hair pencil, fixed against the sides of the goblet: this pressure, though slight, seemed too strong for so delicate a being, it suffered from it, and, notwithstanding the light of two candles, by which we were pursuing our examination, we could perceive issuing from its body a luminous and bluish coloured liquid, of which the traces extended in the water to the distance of two or three lines. This accident did not induce me to leave my hold; I raised it up on the point of the pencil, and scarcely was it placed under the microscope, than it again shed forth a quantity of that cerulean liquid.

I expected that so great an exertion would have weakened it extremely, but I had again the satisfaction to see it apparently full of life, and stirring about with vivacity.

It was not in consequence of the examination of a single specimen that I ventured here to give its figure under a variety of aspects. The abundance with which I was then surrounded, enabled me to sacrifice many, that I might be assured of all the parts of which they were composed; and I examined several which I found next day rather in a languishing state, but which a change of water reanimated. The brilliant liquid of which they have so ample a reservoir, was not even altered; for, having left, during some time, attached to the pencil, one of those which I had destined for examination by the microscope, it spread out a brightness which lasted seven or eight minutes, and was visible, even in full day, to various persons, at the distance of several feet."

Many of the most lively specimens of these animals having been put into fresh water, very clear, and freed from all disagreeable intermixture, they were immediately precipitated to the bottom, became strongly agitated or convulsed, and died in about six seconds. Many of them, while expiring, gave out a quantity of their bright phosphoric fluid.

It seemed absolutely necessary, in order that the insect might exert this power, that it should be in a state of humidity. When the moisture was absorbed, none shone even when bruised. Those which Godeheu had withdrawn from the sea, and placed in the same water in which he had found them, died one after another; but the water in which they had been preserved, shone with a very lively light. A phosphoric matter collected in consequence of this observation, did not, however, last for any time. Three days were sufficient to make it lose its luminous property.

This little animal appeared to be inclosed in, or protected by, a scaly covering, or shell. Its general contour might be said to resemble an almond split down one side, and a little sloped at its superior part. The posterior extremity of its body presented many globules, in the form of a moveable group or cluster. These globules are of a bluish-green, which becomes of a tarnished yellow, in proportion as the animal approaches its end. Godeheu perceived in these grains, the phosphoric matter with which it is provided. We can scarcely doubt that these minute corpuscles are the eggs, and thence their luminous property is the less surprising, since the eggs of many fishes and of several insects present us with a similar phenomenon.

Its superior part is furnished with four moveable antennæ or horns, formed of many articulations, and terminated by tufts of very fine hair. The head is placed in the centre, and armed with some small hooks. Beneath it are two feet, bent, and furnished with hooks, and lower down there occur other organs of movement.

IV. GENERAL SCIENCE.

29. *Blowing Sand.*—On 29th March 1821, in N. Lat. 11° 3′, W. Long. 22° 5′, it was observed by a gentleman going to the East Indies (Mr James Alexander, a cadet), that sand in considerable quantities was found adhering to the upper rigging of the ship. This must have been blown from the shores of Africa, the nearest point of which was at least 300 miles distant from the ship.—*Extracted from* Mr Alexander's Journal by W. W.

30. *Account of the Stupendous Cavern of Watertown.*—There was discovered, about three weeks since, on the north bank of the Black River, upon the land of James le Ray, Esq. opposite to the village of Watertown, an extraordinary cavern or grotto, the mouth of which is about ten rods from the river, north of the Falls of Cowan's Island.

The great extent of the cavern, and the great number of spacious rooms, halls, and chambers, into which it is divided, and the immense quantities of calcareous concretions which it contains, and the different states of those concretions, from the consistence of lime-mortar to that of the most beautiful stalactites, as hard as marble, render it difficult, if not impossible to describe it; and I shall only attempt to give a faint description of three or four rooms.

The mouth of the cavern is in a small hollow, about 5 feet below the surrounding surface of the earth. You then descend 16 feet and a half, into a room about 16 by 20 feet, and 8 feet high, and behold in front of you a large flat or table rock, 12 or 14 feet square, 2 feet thick, and elevated about 4 feet from the bottom of the cavern, the roof overhead covered with stalactites, some of which reach to the table rock. On the left hand is an arched way of 100 feet and 50 feet; and, on the right hand, is another way, 6 feet broad at the bottom, and 6 feet high, which leads into a large room. Passing by this arch about 20 feet, you arrive at another, which leads into a hall 10 feet wide, and 100 feet long, from 5 to 8 feet high, supported with pillars and arches, and the sides bordered with curtains plaited in variegated forms as white as snow. Near the middle of this hall, is an arched way, through which you pass into a large room, which, like the hall, is bordered with curtains, and hung over with stalactites. Returning into the hall, you pass through another arch, into a number of rooms on the left hand, curtained as before, and having stalactites hanging from the roof. You then descend about 10 feet, into a chamber about 20 feet square, curtained in like manner, and hung over with stalactites. In one corner of this chamber, a small mound is formed, about 12 feet in diameter, rising 3 feet from the floor; the top of which is hollow, and full of water, from the drippings of stalactites above, some of which reach near to the bason.

Descending from this chamber, and passing through another arch into a hall, you see, by the side of this, another basin of water rising about four inches from the floor. It is formed in the same way, and has the shape, size, and thickness of a large tea-tray, and is full of the most transparent water.

The number and size of the rooms curtained and plaited with large plaits, extending along the walls 3 feet from the roof, of the most perfect whiteness, resembling the most beautiful tapestry with which the rooms are embroidered, and the large drops of water which are constantly suspended, and points of innumerable stalactites which hang from the roof above; and the columns of spar resting upon pedestals, which, in some places, appear to be formed to support the arches above; the reflection of the lights, and the great extent and variety of the scenery, form altogether one of the grandest and most interesting scenes that can be witnessed. As this cavern has been only partially explored, it is probable that its extent will be found very great.—*Phil. Mag.* vol. lx. p. 71.

31. *Explosion of the Boiler of a Steam-engine at Chester.*—On the 29th June 1822, the boiler of the steam-engine of Mr Boult burst with tremendous violence. It was 8 feet long, 4 feet broad, and 5 feet deep. It was connected with machinery which required steam of great force; and, on the day of the explosion, it was used for the first time, after having been thoroughly repaired. On ordinary occasions, the boiler worked with a pressure of 30 lb. on the square inch; but it was, on the present occasion, exposed to

a pressure of above 100 lb. The steam had been quickly raised to such a degree, that an oscillating movement in the boiler had been perceived for a considerable time. While Mr Boult, with four of his men, were anxiously looking on, it suddenly burst, and destroyed four out of the five individuals, among whom was Mr Boult himself. Mr S. Leet, who examined the boiler afterwards, found the bottom completely torn away, on one side, from the seat of the vessel, and forced into the ash-pit. The plates of iron one-half inch thick, which formed the bottom of the boiler, were twisted from the convex to the concave form, and the two surfaces of the severed plates were comparatively smooth. By this accident, a part of the premises was completely destroyed and in ruins; the windows of the adjoining houses were entirely broken; and a building about fifty yards distant was set on fire by the pieces of ignited fuel which fell upon its roof.—See *Phil. Mag.* vol. lx. p. 67.

32. *Copper raised in Great Britain during the year ending 30th June* 1822.—The following is the aggregate quantity of pure copper produced in all the mines of Great Britain and Ireland, during the year ending 30th June 1822.

	Copper Ore. Tons.	Pure Copper. Tons.		Value of Ore.		
Sold at the Ticketings, in Cornwall,	104,522	9140		£ 663,085	13	2
Sold at Swansea, from Ireland, and various parts of England and Wales,	3,539	388		30,967	12	6
Devon, ...		532	about	46,000	0	0
Sundry private sales, ...		184	do.	16,000	0	0
Anglesea, (estimated at)		600	do.	45,000	0	0
	Tons,	10844,		£ 801,053	5	8

See *Phil. Mag.* vol. lx. p. 75.

ART. XXIX.—*List of Patents granted in Scotland from 13th April to 29th August* 1822.

17. TO WILLIAM CLELAND of the city of Glasgow, in North Brita[redacted]ntleman, for his invention of an "improved apparatus for f evaporating liquids." Sealed at Edinburgh 12t

18. To of Great Marlborough Street, in the

county of Middlesex, musical instrument maker, for an invention communicated to him by a stranger, of "certain improvements in piano fortes and other keyed musical instruments." Sealed at Edinburgh 17th June 1822.

19. To Sir ANTHONY PERRIER, Knight:—For his "invention of certain improvements in the apparatus for distilling, boiling, and concentrating by evaporation, various sorts of fluids and liquids." Sealed at Edinburgh the 9th August 1822.

20. To ALEXANDER GORDON of the city of London, and DAVID GORDON of the city and county of Edinburgh, Esqrs.—For their invention of certain improvements and additions "in the construction of lamps, and of compositions and materials to be burned in the said lamps, and which may also be burned in other lamps." Sealed at Edinburgh the 29th August 1822.

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